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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Mrs. Agnes Ibbetson; W. H. B.; Mr. John Davy; Mr. Grover Kemp; Thomas Forster, Esq.; Luke Howard, Esq.; Dr. Delaroche; W. Moore, Esq.; H. T. B.; Adam Anderson, Esq.; Marshall Hall, Esq.; Mathematicus; Mr. Charles Sylvester; Thomas Stewart Traill, M. D.; Mr John Murray; Richard Lovell Edgeworth, Esq. F. R. S. M. R. I. A. &c.; John Farey, senr. Esq.; Nauticus; and Mr. John Gough.

Of Foreign Works, M. Berthollet; M. Gay-Lussac; M. Thenard; M. Vauquelin; M. Malus; M. Biot; Mr. J. Cloud; M. d'Arcet; M. Decandolle; M. Vitalis; M. Haüy; Prof. Pictet; Prof. P. Prevost; Count de Fourcroy; M. Berzelius; M. Guyton-Morveau; M. Klaproth; M. A. Laugier; M. Chevreul; M. Bucholz; and M. Martres.

And of British Memoirs abridged or extracted, George Pearson, M. D. F. R. S.; Luke Howard, Esq.; the Rev. John Simpson; the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S. &c.; Mr. Bryan Donkin; Mr. J. D. Ross; Mr. George Marshall; James Smithson, Esq. F. R. S.; Mr. Richard Cathery; Everard Home, Esq. F. R. S.; Thomas Andrew Knight, Esq. F. R. S. &c.; Mr. J. Hassel; Mr. William Corston; Dr. William Roxburgh; David Brewster, LL. D. F. R. S. Ed.; B. C. Brodie, Esq. F. R. S.; Mr. William Lester; Mr. William Salisbury; and Mr. Samuel Roberts.

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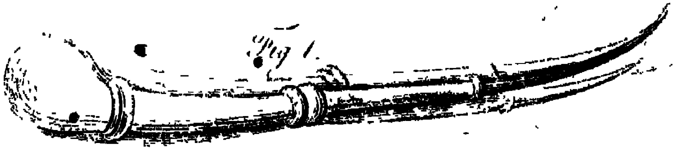
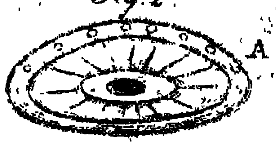


Fig 1.



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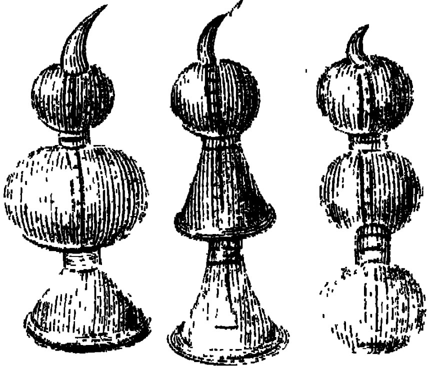

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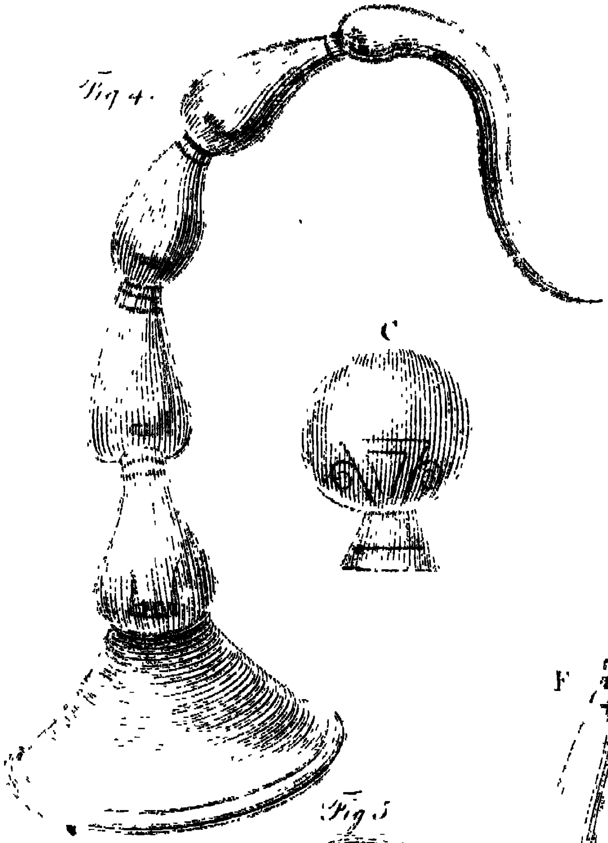

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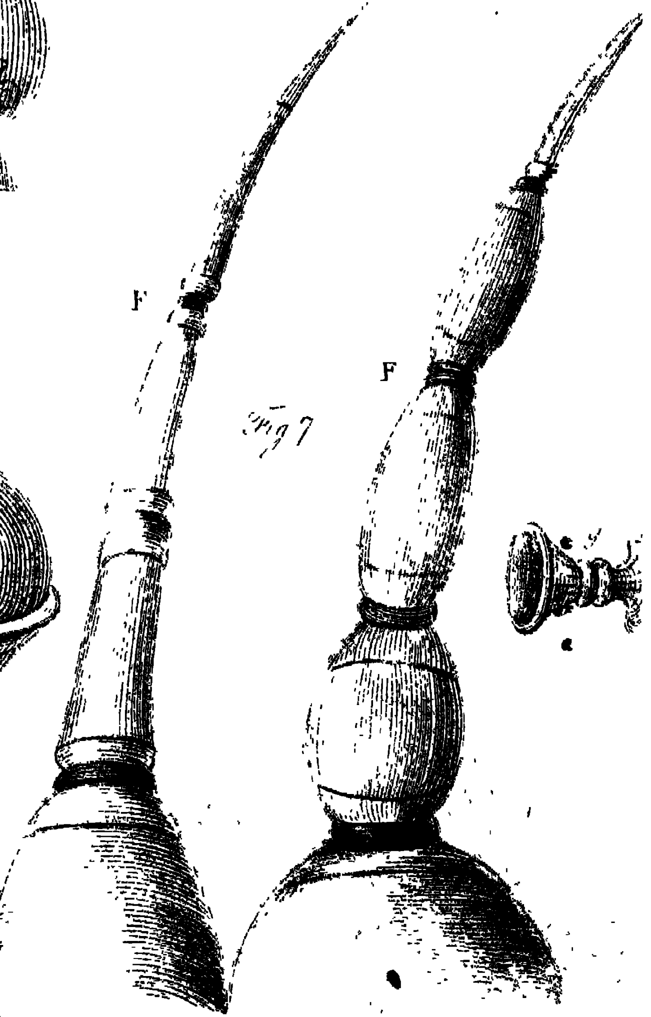

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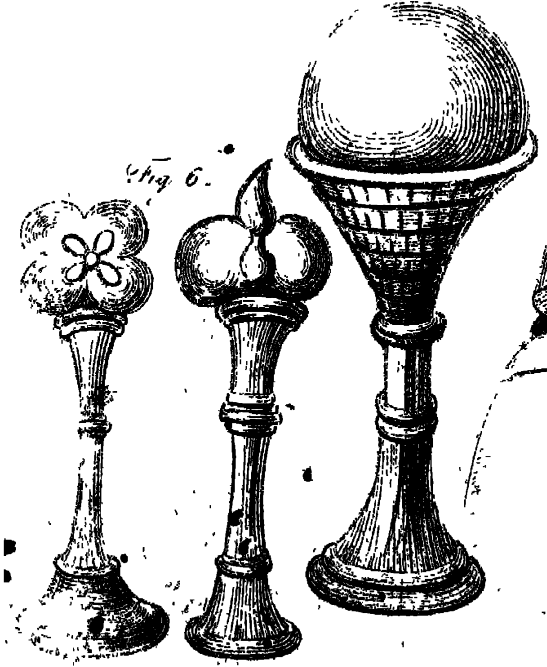
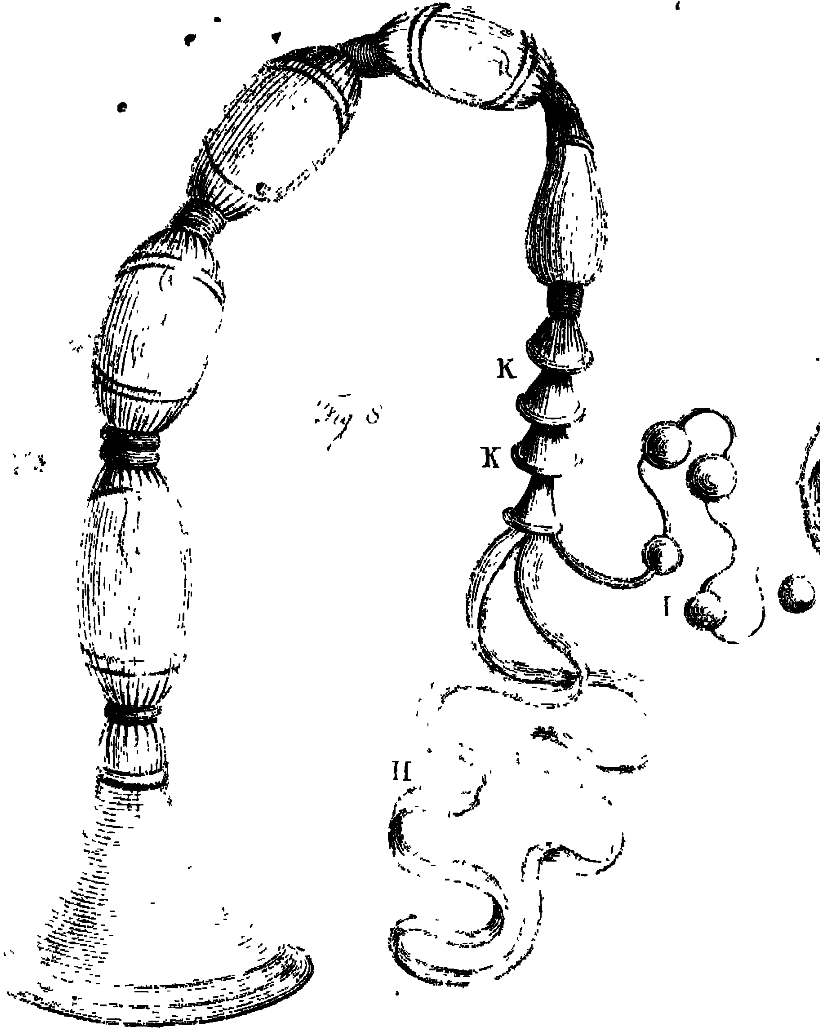


Fig 5

Fig 6.

Hairs in Plants & Cryptogamia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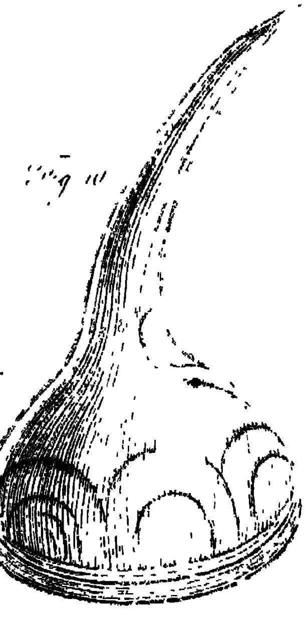
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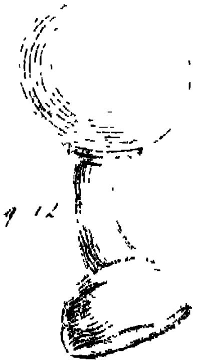
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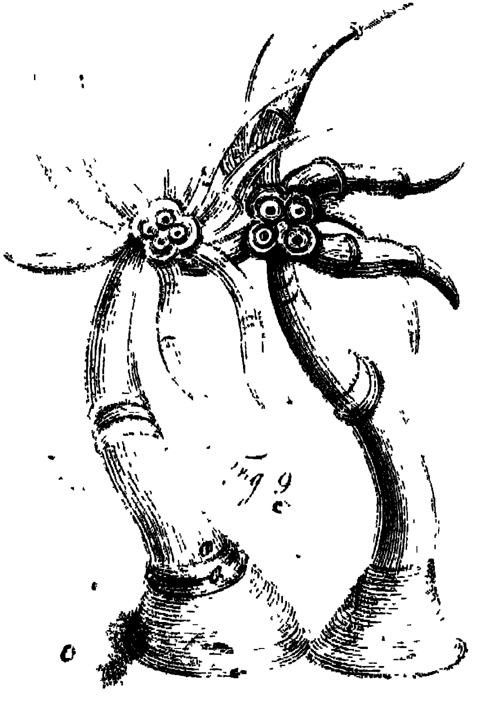
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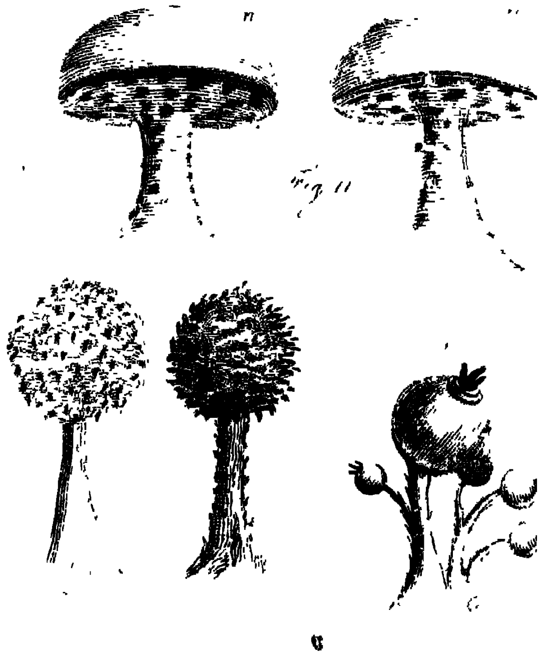
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SEPTEMBER, 1811.

ARTICLE I.

On the Hairs of Plants. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Powerful purposes answered by little means extremely multiplied.

WE study the larger and conspicuous parts of botany, but we leave with a sort of contemptuous neglect all the more diminutive features, as unworthy our notice, little aware how much nature performs in this way, and how many great and powerful purposes are answered by apparently little means, extremely multiplied. If we minutely examine all the works of nature, this will appear a very important truth; nor does any art or science show this more conspicuously than the study of physiology, where all are multiplied little means, conducing to one great and important end. The subject of the present letter will peculiarly exemplify this. It is on the Hairs of Plants.

No perspiration in plants.

I have endeavoured to show, and I hope succeeded in proving, "that the idea of perspiration in plants is an absolute fable," originating from the poorness of our magnifiers;

Source of the deception.

and that all that was taken for perspiration by botanists was one of two things: either 1st, a sort of hair, or instrument in that shape for carrying water to the interior of plants, and performing many of those important services, which their diminutive appearance makes us overlook; or, 2dly, a sort of cryptogamian plant, wholly nourished by the dews of the atmosphere, and proving what they are by passing, like all other plants, from flower to fruit and seed, and showing in each various alteration the concomitant properties of each. That both these appearances have been taken for perspiration there can be no doubt, since I have repeatedly and regularly followed them in every plant peculiarly said to perspire much; and always found it either a fruit or an instrument: and instead of being bubbles of water issuing from the cuticle (as is supposed) their make alone would prove the contrary, as they could not transpire on stalks. Even the vineball is proved to have a stem; and is therefore an instrument, not a bubble.

Another objection.

Before I give a more ample description of these, I shall adduce a farther objection to the idea of perspiration; and prove the impossibility of it by the disclosure of a discovery I long ago made, but would not give to the public, till perfectly convinced of its reality. I have already said, that there is found in the corolla of flowers, and in the stem of trees, a clear transparent skin, which, placed under the most excessive magnifier, shows no alteration of form, nor can any aperture be discovered in it.

The exterior skin of leaves is without pores.

The same is found on the exterior of the cuticle, on each side of the leaf of all plants; so that it is not possible that a drop of water can pass to or from the interior in this way, though certainly air may. It is difficult to clear the skin from all the marks the pattern of the pabulum leaves on it, which were taken by all botanists for the pores in the cuticle. I was once of this opinion; but I have since with such excessive pains laboured to elucidate this subject, and to prepare for the microscope upwards of forty specimens, (cleared in the way described in a former letter;) which in this state were thoroughly examined by myself and others; that there can be no doubt of their being on both sides impervious to moisture. They are divided into small compartments by a narrow vessel; and so extremely fine is the skin, that, when placed in the sliders of my solar microscope, it is only on turning the light in a particular direction while the eye is on it, that it can be discovered. But the double microscope makes it very visible, let it be ever so nicely cleaned and prepared. I am hardly acquainted with any part of the vegetable structure, that plays so many parts, and shows itself in so many ways, as this delicate skin. It was through this transparent skin I saw the dew drop enter the pabulum. It is probably the same skin of which the hairs are formed, which confine not only water but air.

This skin seen in every part of the vegetable structure.

How then can water enter the interior of the leaf, which is thus guarded on both sides by this transparent medium? that water which is often seen underneath the skin of vegetables, and wholly independent of the vessels? it is to the hairs alone they are indebted for it; which, however simple they may appear to the casual observer, are very far from being so in reality. To these indeed plants owe many of the most delicate and important offices, nor can a person see them once, and have a doubt remaining as to their being real instruments formed to effect some curious purpose. To give a faint idea of this astonishing subject is all I can attempt, for to collect a tenth part of the various instruments these hairs are intended to represent would be an endless labour; and to account for the use and manner of acting of a few is I fear more than I can perform well, or as I could wish.

Water enters the leaf through hair-like vessels.

The first idea that occurs on seeing these hairs greatly magnified is, that they resemble the instruments in an immense laboratory. But great indeed must be the laboratory that could show instruments of such contrivance, figures so various, and mechanism so astonishing, even putting their size out of the question. By the most careful attention to their forms, by filling them with coloured liquids, and with art and constant practice learning to manage the heat and light of my solar microscope (opaque as well as common), I have been able repeatedly to fill and empty a few of the instruments, and by these means understand something of their construction. But it is extremely difficult to get a liquid thin enough, as the most trifling degree of thickness chokes the valves. This was the case with extremely diluted ink; still it is to this I owe the conviction of the opening of the

They resemble the instruments of a laboratory.

valves being double. I have also by observation noted the hairs always allotted for certain purposes; but there are many uses it is not possible even to guess at. Innumerable indeed are the offices these hairs perform. To shade from light and heat, to convey moisture, to decompose water, to catch and secure the drops of rain as they fall, and select the dew from the atmosphere, I have often seen them do; but these, I conceive, are but a small part of the offices they daily execute: when an instrument is wanted for the several purposes of carrying moisture to the plants, catching the rain drops on their points, and defending the back of the leaf from the sun's rays, a simple kind of hair is generally used, particularly found on the leaves of trees, as represented Pl. I, fig. 1. This is merely a managed vacuum, which draws the water into the vessel, and thence lets it into the pabulum of the leaf. It is well known, that the backs of most leaves will not bear the scorching sun: and nature has peculiarly formed and adapted the spiral wire, to turn the leaf if so directed. It is not from any great difference in make, for both cuticles are most frequently alike on each side of the leaf; being both composed in part of this clear skin; but the one is pressed down on the pabulum, and is always therefore moist: while the other stands much above it, and, if heated, would soon dry up, peel off, and thus cause the decay of the leaf. When leaves are to be defended from heat alone, and no other purpose to be answered, then, (as in coltsfoot and many other very wet plants) the hairs are formed like a ribbon with a quantity of threads woven round them, and wholly without moisture. But in those which contain moisture, all the different pipes have at the bottom a contrivance for the entrance of the water into the pabulum. This perfect mechanical process I have several times witnessed and described as the dew drops entering the cuticle. See fig. 2, in which the thread *a* contracts or loosens to admit or retain the water. When from a long continuance of sunshine and dry weather in February or March, when the buds of trees are enlarging, and of course much humidity is required for their preservation, a quantity of hairs will be suddenly seen covering all the buds in various directions, the sun cracking the scales, and all the apertures filled

The various uses of the hair.

The reason leaves will not bear the sun on their under surface

Occasional hairs being

filled by a quantity of vessels shaped as at fig. 3. For several years past I have attended with peculiar care to this phenomenon, and noticed the sort of instrument used on the occasion. It never varies, and regularly appears to select the dew from the atmosphere. By four or five in the morning they are almost empty; by eight, perfectly full; again emptied before noon, and late in the evening I have seen them replenished to bursting, or running over: but how they fill themselves, except by means of a vacuum, I have not yet been able to discover. This year all the trees (or rather the buds,) were covered with this vessel, owing to the long drought in March, which never fails to bring it on; it appeared as if all the buds were covered with diamonds.

moisture in droughts.

Buds of trees covered with occasional vessels.

Hairs of odoriferous plants.

In perfumed plants there is a species of instrument that baffles all conjecture as to the manner of its management, or the uses to which it is applied. This is represented at fig. 4. *e* forms a part of it, but is often found separate. The different bells bubble between each division (when part of it is turned to the sun) like a pulse glass when a warm hand is applied to one of the balls: on turning a very hot sun on these, I once blew up two of them; and it not unfrequently happens, that the quantity within the hair, if heat is suddenly applied, bursts the vessels: but it is fortunate when it does so, since they always break at the valve, and by this means discover much of their interior formation. These instruments are mostly found in the balm of gilead, the most perfumed geraniums, and plants that coincide in this respect. When I first saw this, and perceived the divisions to bubble, I was persuaded it was a decomposition of water; but was soon undeceived, for none of it disappeared. I have since repeatedly seen the effect, and been convinced, that it is similar to that which takes place in the pulse glass, and caused by the rarefaction of the air, and the increasing particles of liquid from the admission of caloric among them. Indeed every little power is visible here, nor can any instrument be so fit to try every little variation of temperature, moisture, or evaporation, as these most delicate diminutive ones, which are never idle, as long as the vegetable on which they are placed lives; sensible of every change, even Leslie's differential thermometer is quiet in comparison.

Hairs always break at the valves.

Hairs uncommonly susceptible of alteration.

Fig.

Hairs appropriated for the decomposition of water.

Fig. 5 is the one that appears constantly used for the decomposition of water, where it passes away in a few minutes, with its usual bubbling. I have often seen the same decomposition between the two glasses of my sliders, when exposed to a very hot sun; in short, it is a process so continually taking place, that you cannot make the proceedings of the vegetable world visible to the eye, without a perpetual recurrence of this chemical work: such a quantity of hidrogen is wanted, not only for the juices of the bark, but for the seed, inflated with it, that the process must of course be perpetually going on. In describing the various sorts of instruments I have observed, I have given two or three that strike as most singular; but they are in such numbers in plants, and so various, that I have found it difficult to select them. It is not uncommon to see several different sorts of instruments on the same plant, apparently appropriated to a variety of purposes, nor is it possible to mistake the fruit for the instrument: the latter so much resembles the purest crystal, and their forms are so extraordinary, their valves so truly mechanical, that no person can see them, and take them for any thing but what they are, "an instrument;" nor did I ever show them without exciting an exclamation of surprise. I have once or twice found them inflated with a green liquid; but this is very rare. This is the case in the love-apple. What in that plant was supposed to be perspiration is a small instrument of this kind (see fig. 6).

The cryptogamian fruit cannot be taken for the instruments.

The most extraordinary instruments found in small plants.

Extraordinarily figured hairs are rarely to be found, except in herbaceous annuals, or small plants. The wild plants exceed the cultivated in assistance of this kind. It would seem, that, when art lends her aid, nature is less attentive to the preservation of her nurslings: though I believe it requires many years cultivation to lose any of them, still I have found occasional hairs oftener on wild plants than on garden ones, and double flowers almost banish them. Trees and shrubs have seldom any but simple formed hairs, if those with double cases or valves deserve this epithet; but occasional assistance of this kind is perpetually found, particularly among exotic trees. Nor are hairs found often on evergreens; they would undoubtedly burst with the first frost of winter. The firs also are void of all assistance of this kind,

Extraordinary hairs seldom found in double flowers.

kind, except now and then on the calyx of the leaves: and then I have observed, that the water gets mixed with the resinous juice of the bark, which coagulating bursts the pipes. These hairs are also very different from those which are fixed to the flying seeds, &c., for they resemble the coralines, and the bones of fish; indeed the exact likeness of these three different objects is very striking and curious. The hairs which surround the buds of trees, and are generally wound round them, are never inflated till wanted, and till a certain time in the formation of the bud: when black (as in fraxinus excelsior, juglans regia, and many others), the valves are admirably seen to open and shut in a large magnifier, admitting and passing the water through the black lines.

Hairs alter their forms.

That the hairs alter their forms, I have many proofs. During great drought I have seen those, which were before plain pipes, swell into divisions between the valves, changing their form from that at *e* fig. 7, to that at *f*; and plainly proving the shape of the valves to be as fig. at *g*. On placing fig. 8, Pl. II, in the solar miscroscope, after great bubbling and confusion, I took it out, and found the ribbon changed from the appearance it has at *h* to that at *i*. It appeared as if it had been before inclosed in another case, which case had melted away with the heat of the sun, and left the inclosed balls and string uncovered. I have so often seen the same result from repeatedly placing it, that I cannot doubt that this is the case. The divisions *k k* are often found attached to different shaped instruments, ending sometimes in bells, sometimes in plain pipes; contracted, or inflated, as the occasion requires. Nothing can be more common than fig. 9, which is always full of water; and fig. 10, which is found on the galium aparine. Extraordinary as is all I have related, it is not more wonderful than true. I am the first person that may be said really to have turned the solar microscope on the botanical world; is it then incredible, that I should have wonders to relate? did any person ever take a miscroscope in hand without it?

Description of the cryptogamian plants.

I shall now turn to the cryptogamian plants, equally taken for perspiration, and described by all botanists as such. Many of them resemble the powdered lichens, when they begin

Fruit known by its various changes ending in seeding.

to go to seed, though at first appearing like a drop of water, which, even while your eye is on it, turns white, and soon becomes hard and firm; changing to seed. These are found on the mint, the pea, and innumerable other plants, said to perspire much. That which Hales took for perspiration on the leaves of the sunflower is a sort of mushroom, extremely moist, shown at fig. 11, *w*; and that on the vine, fig. 12: but I must stop, or my sketches would never end. I observe that the cryptogamian plants on the rose, and many other plants, because red, are allowed not to be perspiration: but surely the proof is not in colour, but on the matter passing from flower to fruit and seed, which all this sort does in a day or two; yielding generally a sort of sirup, and equally nourished by the dews of the atmosphere: and certainly equally unfit with the hairs to be reconed perspiration. I flatter myself therefore, that this will serve to convince those who still doubt.

Innumerable offices the hairs perform.

If I were to mention all the different offices to which the hairs are applied, it would be endless. To catch, convey, and mix, the powder of the stamen with the sirup of the pistil, they are peculiarly adapted, having in each hair a duct for conveying the mixed juices, when melted, to the canal in the pistil. All this is plainly seen, since in the solar microscope each hair is as large as a walking-stick. How many various offices do the hairs perform in the corolla, calyx, and stipula! There is one peculiarly appropriated to this latter part, in all diadelphian plants, most curiously formed. How wonderful is the hair in wet plants! placed to guard the air vessels from being filled with insects, they exactly resemble swords, shoot in a circle and meet in the middle of the vessel as at fig. 7. How many an insect and water-fly have I seen run through by them! But this is not all, they have a sort of spring, which makes the hair strike down, and thus get rid of the creature it has threaded. When I give my letter on water-plants, I shall show the mechanism of this hair, which is as wonderful as any of the preceding account.

Different from the armature of plants.

This subject should not be made to interfere with the armature of plants, which is wholly of a different nature, and consists but of two sorts of thorns; the 1st like those of the rose, the acacia, the gooseberry, &c., is formed entirely of the rind, and an excrescence of it; probably arising from an extreme tendency in that part to grow in the same manner as the quercus suber, the ulmus campestris, and many others, the rind of which is a sort of cork, always increasing. The 2d sort of thorn is that which in the cratægus is a disorder in the tree, to which some plants are peculiarly subject: a sort of missed bud, from the stoppage of the line of life, caused probably from the momentary check of the juices, on some sudden alteration of the weather; as I have observed, that, when the barometer and thermometer are without much variation, except the natural one of day and night in the latter, no thorns come out. I have measured at such a time a shoot three quarters of a yard long, without a thorn. But when in the spring alterations are frequent, the branches will be scarce two inches, and always ending in a long one: and on dissecting this, the line of life will be found to have stopped, before any other part of the plant.

Hope to introduce the subject more in detail.

I intended to give merely a sketch of this subject, till I better understand how to inflate the hairs with a coloured liquid, and till I can more thoroughly comprehend their uses and management; for this indeed I should have waited, but that it was absolutely necessary to prove, that I would not have written against the perspiration of plant, without a complete conviction of the truth of my assertion: "that the whole system of perspiration could not be supported against the absolute proof the solar microscope adduces of its falsehood." If I were rich, I would certainly have the instruments imitated in glass, properly magnified (if it could be done) as I think much might be learnt from it. It is the mechanism of nature: we talk much of its simplicity, but it surely consists only in not making use of more contrivance than is necessary; and when the mechanic powers are wanted, can we do better than study them from models so perfect, forms so wonderful? and though we could not succeed in forming a sort of air pump in a hair; yet it might serve to teach us to simplify our machines, and to rectify many of our mistakes.

Your obliged servant,
AGNES IBBETSON.

Cowley Cottage,
July 29th, 1811.

II.

II.

Inquiry concerning the Natural Economy of Ants. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

Inquiry concerning the natural economy of ants.

HAVING always observed, since I first commenced taking your excellent publication, that you have constantly paid the kindest attention to the inquiries of such as wish to be informed on the interesting subjects embraced by your plan, I feel almost confident of your permission to request, through the medium of your journal, the communication of such original facts and observations relative to the natural economy of different species of ants, as may have occurred to the notice of any of your numerous readers. It appears to me, that, striking as the habits of this genus of insects certainly are, the subject is, as yet, by no means generally well understood.

Your compliance with my request will be considered a particular favour.

I am, Sir,
Your obedient humble servant,
W. H. B.

III.

Report of a Committee, consisting of Messrs. Berthollet, Chaptal, Vauquelin, Le Breton, Vincent, and Guyton-Morveau, appointed by the Institute to inquire concerning the Process of the late Mr. BACHELIER, *for the Composition of a preservative Stucco*[*].

Speedy alteration of the stone used for building at Paris

IT was in 1755 that Mr. Bachelier, struck with the speedy alteration of the stones employed in the principal buildings at Paris, and the inconveniencies of the process employed from time to time to renew their surfaces, proposed to the

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[*] Abridged from the Mag. Enc. Dec. 1809, p. 241.

superprevented by a composition,

superintendant of the royal buildings to try a preservative stucco. Accordingly three pillars in the court of the Louvre were coated with this stucco for half their length, two facing the south, the other the west. These were still remarkable in july last for the uniformity of their tint, strongly distinguished from the dull gray and earthy aspect of the contiguous parts: but as the alterations made in completing the Louvre would necessarily destroy every trace of this experiment, the Institute appointed a committee to inquire concerning it, before it should be too late.

too thin to injure the finest sculpture,

and unaffected by the weather.

In company with Mr. Fontaine, architect of the Louvre, the gentlemen abovementioned examined the pillars, and found, that the stucco applied formed a coat too thin to injure the finishing of the most delicate sculpture; that it retained a uniform colour even in the parts exposed to the action of the wind, rain, and sun; that rubbing it with the hand made no impression on it; and that, if one of the three pillars exhibited a reddish yellow tint, there could be no doubt, from its appearance in other respects, that this was owing to some colouring matter added intentionally.

Account of it from memory by the inventor's son.

It could not be found on inquiry, that Mr. Bachelier had consigned his process to writing, and the following was the best account his son could give of it from memory. "Its basis consists of the sifted powder of oystershells, previously washed and calcined to whiteness, mixed with the butyraceous and caseous part of milk. My father used the common cheese known by the name of *fromage à la pie* [skimmed milk cheese?]. He first separated all the wheyey part by pressure, and then left it ſome time exposed to the air to dissolve or soften. In this state he mixed with it a quantity of calcined oystershells in fine powder. When this mixture was brayed on a stone, the cheese softened, and formed a very smooth and whitish liquid paste. To make the stucco he diluted this with a solution of alum in water; the quantity of water being proportioned according to the thickness of the coat intended to be applied."

Paper coated with it from which writing could be effaced.

Mr. Bachelier could say nothing of the proportions of the ingredients, he only added, that, his father having thought of employing this composition undiluted to cover leaves of paper, from which writing was easily effaced by a wet sponge, he observed, that the oystershell powder was taken at random, and added to the cheese till it had acquired the consistency of a paste capable of being spread on paper.

Oxide of lead in it.

The committee having obtained from Mr. Bachelier a few leaves of paper covered with thin paste, found from the very deep black immediately given it by the hidrosulphuret of potash, that it contained a considerable quantity of oxide of lead, the presence of which there was no reason to suspect in the preservative stucco, so that they could not be considered as the same.

Analysis of the stucco.

It remained therefore to analyse the stucco, which was done by Mr. Vauquelin; though, as a very small quantity only could be obtained by scraping the pillars, it did not admit of repeated trials. The results of his analysis gave

Carbonate of lime	63
Sulphate of lime	7·73
Carbonate of lead	6
Oxide of iron, about	4
Silex	2
Water	20
Organic matter, an indeterminate quantity	
	102·73

The surplus of 2·73 Mr. Vauquelin ascribes either to the matter not having been dried to the same degree, or to the escape of a little carbonic acid during the calcination.

No animal matter found, but the pungent smell of burning vegetables emitted.

The presence of animal matter was sought for, but not a particle could be separated. The smell it emitted during calcination no way resembled that of aminal matters; on the contrary it had the pungent sharpness of vegetable substances. On being exposed to the action of heat in a retort however, a clear and almost colourless liquid came over, from which potash expelled a very evident ammonical vapour. This indicates, that some animal substance entered into the composition, but that in time it was decomposed, and left only an ammonical salt. The brownish colour it acquired in the fire also proves, that some animal matter still remained in it; though altered in its nature, since it neither emitted the smell proper to such substances, nor yielded any preceptable quantity of oil. Lastly,

Indications of animal matter.

No alumine.

Lastly, this matter yielded no appreciable quantity of alumine, so that it may be presumed no alum was employed in the composition.

Analysis of the paper coating.

Mr. Bachelier having some of the paper that had been prepared by his father, the coating of this was analysed, and the result indicated, that

Quicklime	56·66
Calcined gypsum	23·34
Ceruse or carbonate of lead	20

had entered into its composition.

This more to be depended on.

On these proportions more dependance can be placed than on the former, since it was impossible to detach the plaster from the pillars without some of the substance of the stone itself.

Cheesy matter the vehicle.

That the caseous part of the milk is the proper vehicle for the powders we learn from the positive testimony of Mr. Bachelier, the son; and its utility is confirmed by the experiments of Mr. d'Arcet published some years ago*.

Efficacy of the compound unquestionable

Of the efficacy of Mr. Bachelier's composition there can be no doubt, as we have irrefragable and still existing testimony of it; nor would it be difficult to estimate this beforehand, when we consider the causes, that produce the gradual decay of the finest buildings in this capital, and the means of guarding against them.

Stone not liable to this alteration,

Hard and fine grained calcareous stone, susceptible of a greater or less degree of polish, is not liable to this alteration. It is therefore owing to the nature of the stone commonly employed, which is of a loose and unequal texture, filled with cavities, and found by analysis to contain 10 or 12 per cent of silex, and frequently 3 or 4 of oxide of iron. The difference of the stones from the quarries near Paris is evident from the tables of Mr. Rondelet, in his Treatise on the Art of Building; where we see, for example, that what is called the *grignard* of Passy is of the specific gravity of 2·462, and supports a weight of 6750 kil.; while the *lambourde* of St Germain has only 1·560 sp. grav., and is

and liable to it.

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* Déc. phil. an X, No. 5. The pamphlet entitled *l'Art de peindre au Fromage, ou en Ramekin*, which Mr. d'Arcet regrets his being unable to procure, was foreign to the subject, as it related to painting with soap or wax. [See Journal, Vol. I, p. 212.]

crushed by a weight of 921 kil. The prices of these two kinds of stone differ too in the proportion of 26 to 10.

Spiders form their webs on these stones.

It is not at all strange, that the little spider called by Linnæus senoculata, the cellar spider of Geoffroy*, should find on the surface of this stone a convenient situation to shelter itself, deposit its eggs, and spread the nets in which it awaits its prey. Its web extends circularly round the cavity, that serves as its den, forming round spots of 3 or 4 cent. [1 in. or 1½] radius. It is not thirty years since the *hotel des monnoies* was built, and I have counted no less than sixty-eight of these dark gray spots on one of the pillars of the vestibule. Similar ones are found not only on the stone, but on the coatings of plaster, and on the walls covered with common stucco. It is particularly in the joints and angles, that the insect begins to fix itself. I have seen several on walls, the stucco of which had been coated afresh within less than seven years. These spots at length form a continued coat, retaining the sloughs of these insects, the remains of those on which they feed, and the dust raised by the wind, so that lichens soon take root in them.

Mode of preventing this.

If it be asked, how is this to be prevented? the answer is easy. By a composition that resists water, will adhere to the stone so as not to scale off, has a sufficient degree of consistency to stop the pores accurately, is liquid enough to be spread as a wash, and uniformly to ice over, as it were, all the saliant and indented parts, without thickening the angles or blunting the edges, and lastly which gives to the assemblage of coarse grains the smooth surface of polishable stones, in which it appears these insects cannot nestle. And this we think may be expected from Mr. Bachelier's stucco.

Other means.

Meantime I must observe, that, in the present state of our chemical knowledge, other means of fulfilling these conditions may be pointed out. We know for instance, that phosphate of lime is one of the most fixed combinations: it would be sufficient therefore, to wash over the stone with phosphoric acid more or less diluted, or with phosphate of lime, lead, magnesia, &c., held in solution by an excess of their acid, to give it a sort of covering, that would render it as unalterable as the stone of Logozan in Estramadura. It is equally known, that sulphate of barytes resists all agents in the humid way; and we might certainly coat the stone with this earthy salt, by first impregnating it with a solution of sulphate of iron, zinc, magnesia, alumine, &c., and immediately washing it over with barytes water*. The insolubility of oxalates and tartrates of lime, and the adhesion they contract by deposition even on polished substances, suggest processes for washes not less solid; as the acids added to these salts to render them temporarily soluble, saturating themselves with their base from the substance of the stone itself, would not fail to connect together all the grains, fill up their intervals, and completely close the pores. Trials made with a view to ascertain the justice of this reasoning have confirmed the expectation of a successful result; since on the most porous stones they have produced a surface, on which the eye could see no appearance of coating, but which, being rubbed with wet black cloth till the cloth showed signs of wear, was not in the least soiled by it.

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* Mr. Latreille informs me, that he has found the same habits in Lister's *aranea atrox*.

The latter too expensive.

Preparations of this kind however would be much more expensive than Bachelier's stucco, so that their use must be restricted to the preservation of sculpture of extreme delicacy.

General results of experiments.

For farther satisfaction trials have been made with different kinds of stone, and stucco made in imitation of Bachelier's. These have given rise to the following observations.

1. All the compositions in which alum water was employed soiled the fingers, and were washed off by water.

2. The cheese that acquires the greatest consistency with dry substances is that which is almost entirely deprived of the butyraceous and wheyey parts. Mr. d'Arcet, in the paper already quoted, had remarked, that these were more detrimental than useful, that painting with milk would not resist water, and that the cheese called *fromage à la pie* might

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Filtering stone spoiled by accident.

* Accident furnished Mr. d'Arcet with a striking proof of the readiness with which this change of bases by superior affinity will fill the pores of the most porous stones. A capsuleful of strontian water happening to be overturned into a filtering stone, it never after let through a single drop of water.

be used after it had grown dry, though less advantageously than when fresh made and well drained.

3. A mixture of this cheese with lime simply forms a paste, that adheres but slightly even to coarse grained stone, and not at all to paper.

4. Calcined gypsum, which in a small dose facilitates the union of the lime and cheese, renders the paste hard and clotty, if it be used in too large proportion.

5. It had appeared, that whiting, which is used in paper hangings, might be admitted into the preparation: but it was found, that, if this earthy substance, which in a process described by Mr. d'Arcet is carried to twenty times the weight of the lime, may be used with success for inside work, it would make too thick a coat, and would not adhere so strongly to the stone.

6. The addition of a very little ochre, or red oxide of iron, to this preparation, will give it such a tint as may be wished, without altering its properties.

Proportion of cheese.

The proportion of cheese must depend in some measure on the state in which it is, and cannot be determined precisely but by the condition of making a soft paste. A fourth of the weight of the solid matters appears to be a sufficient quantity of cheese fresh from the press.

Preparation of the compound.

The quantity of lime to be used at once being determined on, it is to be slaked in as little water as possible, but enough to make it pass through a sieve not very fine, in order to separate the parts that will not slake. This is to be triturated with the cheese to the consistence of a soft, smooth, and coherent paste. To this are to be added the calcined gypsum and the white lead, which must not be adulterated with chalk, and by farther grinding on the stone with a little water the whole is to be reduced to a pap, rather thick than fluid. Lastly it is to be diluted with common water the moment of using it, which is to be done with a painter's or varnisher's brush.

IV.

Observations and Experiments on Pus. By GEORGE PEARSON, *M.D. F.R.S*[*].

Properties of pus not determined.

CHEMICAL writers vary in their statements of the properties of pus; and they consider, that a farther investigation is requisite for the purposes of science. Physicians confess, that, in numerous cases, they cannot form a satisfactory judgment of the nature of diseases, on account of not being able to determine what is, and what is not purulent matter; likewise probably, on account of the existence of different kinds, or varieties, at least, of this substance, afforded by different disorders.

I beg leave, therefore, to submit to this learned Society, my own observations, experiments, and reasoning on this animal matter.

SECTION I. *Simple, and obvious Properties.*

Different kinds of pus.

The different kinds of fluid, commonly considered to be pus, may be distinguished by the following titles:

I. The creamlike and equally consistent.
II. The curdy and unequal in consistence.
III. The serous and thin kind.
IV. The thick, viscid or slimy.

Properties of the 1st kind.

I. A pint of the first sort was taken out of the pericardium, after a fatal inflammation of the heart, in St. George's Hospital, and obligingly sent to me by my colleague, Dr. E. N. Bancroft.

The *colour* was yellowish—the *smell* was fleshy when warmed—it was smooth and unctuous to the touch.

2. The specific gravity of two different portions was as 1630 and 1633, that of distilled water being 1580; each substance being of the same temperature. Serum of the blood of different patients, was found at the same time to be 1626, 1627, and 1630. Accordingly, the distilled water being 1000, the pus is 1031, and 1033; and the serum is 1029, and 1031.

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[*] Philos Trans. for 1810, p. 294.

3. After 12 hours repose, about two ounces by measure of a limpid fluid having appeared on the top, it was decanted from off the opaque purulent fluid; which was become thinner in the upper part of the vessel containing it, and thicker in the lower than before.

4. On farther repose, it did not become offensive so soon as a portion of the same pus mixed with a little blood, or as serum alone.

5. This pus neither indicated acidity nor alkalescency to the usual tests, viz. turnsole paper, tincture of red cabbage, Brazil-wood paper, and turmeric paper. I have, in other instances, sometimes observed acidity to be indicated by turnsole paper; but in none alkalescency, so long as the matter remained without fœtor.

6. Being examined under the microscope, when duly diluted with distilled water, innumerable spherical particles were seen, which did not appear altered in figure, or diminished in number, by extreme dilution; that is, they did not appear to have been dissolved.

of the 2d kind, II. A pint of pus of the second kind, *viz. curdy*, was afforded by a psoas abscess.

The colour was brown. It felt knotty. On pouring from one vessel to another, the curdy masses were manifest, and of various sizes, from that of a pin's head to a hazel nut. It was more viscid than the former, and of a little greater specific gravity. On standing, a limpid fluid appeared upon the top, as in the first kind, but in smaller quantity. Globules were seen with the microscope, but also a number of irregularly figured larger masses. Putrefaction took place sooner than in the former kind. In other properties, this pus was similar to the first kind.

of the 3d kind, III. *Serous thin pus.* It was produced by a fatal inflammation of the peritoneal coat, without ulcer, and taken out of the cavity of the abdomen. A good deal of serum was also effused, of which the pus was a deposit. It was not much thicker than milk. To the feeling it was not at all unctuous. The smell was slightly offensive. On standing 24 hours a sediment appeared, occupying only one half the full vessel, under a wheylike liquid. Putrefaction took place sooner than in either of the two former kinds. The specific gravity was the same as that of the first sort. In other properties it was similar to the creamlike pus above distinguished.

and of the 4th.

IV. A pint of the *viscid pus* was obtained from an abscess among the muscles of the thigh. If I had not had entire confidence in Mr. Brodie's accuracy, who was so obliging as to attend to my request, on this and many other like occasions, I should have supposed, that this was expectorated matter, it so exactly resembled in its simple properties the *ropy kind*, described in a paper on expectorated matter. Phil. Trans. 1809, P. II, p. 317*.

The appearance was not quite uniform, there being semitransparent masses in small proportion, mixed with the perfectly opaque white matter. It was almost inodorous. To the touch it was quite smooth. The specific gravity was nearly that of the second kind of pus.

On standing 24 hours, about one ounce measure of limpid fluid rose to the top of the whole mass. Putrefaction did not take place so soon as in expectorated matter of the same consistence.

The examination by the microscope manifested innumerable spherical particles among leafy masses, and numerous particles of irregular forms.

The simple properties were otherwise similar to those of the other sorts of pus, above distinguished.

Other differences.

Many other differences of purulent matter are universally recognized; but they are either varieties of the four kinds already named, or the differences depend upon the obvious mixture with adventitious substances; such as the red part of the blood, coagulated lymph, serum, putrefied matter, fibrous and membranous masses, calculi, &c.: therefore, I deem it useless to describe them.

SECT. II. *Agency of Caloric.*

Action of heat on pus.

1. The above kinds of pus coagulated like serum of blood, into a firm, uniform, soft solid, at the temperature of 165° completely; but partially at 160° of Fahrenheit's thermometer.

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[*] See Journal, Vol. XXV, p. 220.

2. The decanted limpid fluid from pus, Sect. I,—I, II, III, IV, coagulated completely into a firm uniform mass, like serum of blood, at 165°, but it became opaque and thickened at 160°. By pressure of the firm curd thus produced, a watery liquid was separated, which on due evaporation did not give a jelly, but was coagulable like the decanted liquid just mentioned.

The thick opaque matter, after decanting the limpid fluid, coagulated as before said, into a firm mass at 165°.

Evaporated to dryness.

3. Each of the above four kinds of pus, being evaporated to dryness, left in no case less than one tenth of its original weight, or more than one sixth; but most frequently one seventh or one eighth of brittle matter. The smallest proportion of residue was left by the 3d, or serous kind; the largest, by the 2d or curdy. These residues generally became rather soft, especially those of the 3d, or the serous kind, after exposure to the air.

4. The opaque part of pus after separating the limpid fluid afforded on evaporation from $\frac{1}{15}$ to $\frac{1}{30}$ more of brittle residue, than an equal weight of the pus itself; and it remained hard on exposure to the air. The limpid fluid, evaporated to dryness, yielded about one tenth of brittle residue; which grew moist, and sometimes deliquesced, on exposure to the air.

Residua.

5. The brittle residues above mentioned (3), being exposed to fire in platina crucibles, flamed for some time, emitting a very offensive, pungent, empyreumatic smell; the uninflammable residue being kept in a state of ignition for a longer period, what remained at length was fused readily from the *serous, viz.* the third kind of pus; but in the cases of the other exsiccated residues of the 1st, 2d, and 4th kinds of pus, they barely were melted, or only became soft and claggy. The fused residues from the *serous pus* amounted to $\frac{1}{30}$ or $\frac{1}{35}$ of the exsiccated pus; and to $\frac{1}{250}$ or $\frac{1}{300}$ of the original purulent matter. Those from the second kind, the *curdy*, amounted to $\frac{1}{50}$ or $\frac{1}{60}$ of the dried matter, and to $\frac{1}{350}$ or $\frac{1}{400}$ of the pus itself. The fused masses from the 1st and 4th kinds of purulent matter afforded intermediate quantities of melted matter between those just mentioned.

6. The fused residues (5), being treated in the manner described in a former paper, Phil. Trans. 1809, P. II, p. 326—329 *, I found they consisted chiefly of muriate of soda, phosphate of lime and potash; with strong indications of carbonate of lime, and a sulphate; beside traces of phosphate of magnesia, oxide of iron, and vitriſiable matter, probably silica. On a reasonable calculation, it appeared, that in the *serous kind* of pus, the muriate of soda amounts to from one and a half, to two per 1000; the phosphate of lime from one, to one and a half per 1000; the potash from one half, to three fourths of a part in this quantity; and the other matters together, to half a part in 1000. In the *curdy matter*, the second kind, the muriate of soda amounts to from three fourths of a part, to one in 1000; the phosphate of lime to one; the potash to less than one half; and the other matters united, to half a part in 1000. The first kind of pus, the *creamlike*, and the fourth, the *viscid*, afforded from the melted residue the same substances as the *serous* kind, excepting a somewhat smaller proportion of muriate of soda, and potash.

7. The brittle residues of evaporated pus, after decanting the limpid fluid (4), being treated with fire as above related, the remaining matters were melted with more difficulty, and less completely, and contained a smaller proportion of muriate of soda and potash than the original pus.

8. The decanted limpid fluids (4), being evaporated to dryness, these residues were exposed to fire. They were melted, and then afforded a larger proportion of muriate of soda and of potash, than the pus itself; but with the same proportion of the other saline and earthy substances.

SECT. III. *Agency of Water.*

Action of water on pus.

1. After decanting the limpid fluid from off half a pint of the four kinds of pus as above related, (Sect. I,) three ounces by measure of distilled water were mixed with each of them. After 48 hours repose, a limpid fluid of nearly the quantity of two ounces by measure was seen

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* Journal, Vol. XXV, p. 227—229.

forming an upper stratum to the pus. It was decanted for examination.

(*a*) On exposure to fire it became turbid like milk, as soon as the temperature was elevated to 165°, but did not become thicker at a greater elevation.

(*b*) On evaporation to dryness, the residue amounted to about one fifteenth of the weight of the liquid from the serous pus, and to one twentieth from the three other kinds; in place of about one tenth, as from the first decanted liquid, (Sect. I, 4); and as from serum of blood. The residuary matters were of the same kind as those above described, Sect. II, 2—6.

2d solution. (*c*) Three ounces by measure of distilled water having been again mixed with each of the four kinds of pus, and, in 48 hours, two ounces measure of decanted limpid fluid from each having been evaporated to dryness, residues of the same kind, in the same proportions, and in nearly the same quantities as before, were obtained (*b*). These decanted fluids became nearly as turbid as the former, on raising their temperature to 165°.

3d solution. (*d*) Distilled water was added a third time, in the quantity of eight ounces by measure, to each of the four parcels of pus under examination; and, after 48 hours repose, six ounces of limpid fluid were poured off from each of them. At the temperature of 165°, the decanted fluids became turbid; that of the serous pus more so than the others. On evaporation to dryness, a much smaller quantity of residue was obtained than before, viz. one sixtieth from the serous pus, and one seventieth from the others; and it consisted of the same kind of substances as above described; but the muriate of soda and potash were in smaller proportion than before.

4th solution. (*e*) A fourth time distilled water, in the quantity of a pint, was mixed with the present four parcels of pus; and, after standing 48 hours, three fourths of a pint of clear colourless liquid was poured off from each of them. It became slightly turbid and whitish on boiling. On evaporation, each parcel afforded about $\frac{1}{90}$ of the fluid employed. The residues now consisted of animal matter, with a much smaller proportion than before of muriate of soda,

phosphat

phosphate of lime, and potash—nothing else could now be traced.

5th solution.

(*f*) Distilled water, in the quantity of a pint, was once more mixed with the four sorts of purulent matter undergoing inquiry. After 48 hours, a pint of liquid was decanted from off each of them; but being slightly turbid, they were left to stand 24 hours. By this time a sediment was deposited from each of the liquors; but being still, though very slightly, turbid, they were filtrated through suitable paper. They were then transparent. The transparent filtrated liquors had their transparency disturbed by a boiling temperature. They became also slightly milky with nitrate of silver, but scarcely so with infusion of gall nut. On evaporation to the quantity of an ounce from each pint, the residuary liquids appeared slightly globular. These, on evaporation to dryness, yielded not more than one part of animal matter, from each 500 of the transparent filtrated liquids.

Residuum.

(*g*) On standing three or four days in a cold room, the parcels of pus, after the ablutions just related (*a—f*), exhibited a whey coloured liquor at the top, of which about ¼ of a pint was poured off from them. More turbid liquor was also separated from the washed pus, by pouring it upon a porous cotton cloth strainer, which left purulent matter of the consistence of starch mucilage, amounting to about one half the original weight.

Its properties.

(*h*) The pus freed from coagulable limpid liquid by repeated ablutions (*a—h*) was white as snow—equal in consistence—perfectly smooth—the 4th kind was less viscid than before, but the others were more so—no smell—not at all disposed to putrefy—on elevating its temperature to 165° and higher, it did not coagulate into one mass, nor into clots, or large masses of curd, but a watery fluid separated from a fine soft somewhat curdlike opaque fluid; which did not become more curdy, even on boiling—it did not appear that above a grain of this part, or state of pus, dissolved in 1000 waters—was highly globular under the microscope, and remained so, although coagulated by nitrate of silver; by infusion of gall nut; by alcohol; and supersulphate of alumina—with muriate of ammonia, nitrate of potash, and other neutral salts, and with carbonate of potash, it produced a viscid semitransparent mass like expectorated half transparent matter—exposed to fire in a platina crucible, it was inflamed, but did not emit an offensive smell, and after continuing the ignition, the residue was a particle of half fused matter, not amounting to $\frac{1}{3000}$ of the pus after ablution, nor above $\frac{1}{100}$ of the same matter exsiccated; it consisted of phosphate of lime and vitrified matter—no ammonia was perceivable, on mixing lime with this washed pus; nor muriatic acid on adding sulphuric acid.

Different kinds of pus agitated in water.

2. (*a*) A tea spoonful of the *creamlike pus*, being agitated in half a pint of distilled water, produced a milky fluid, with a number of small curdy particles suspended, but very few leafy or fibrous pieces or clots.

(*b*) The *serous pus* being treated as juſt mentioned (*a*), the same appearances ensued.

(*c*) The *curdy pus* being agitated in the same manner in water, a number of clots, leafy, and fibrous masses, were seen suspended among fine small curdy particles in a pearly liquid.

(*d*) The *viscid pus* being treated as just said, it required long continued and violent agitation, to diffuse it through the water, and then the appearances were as last described.

Boiled.

3. Pus of any kind, after boiling in twenty times its quantity of water, was quite as globular under the microscope as previously. With a smaller proportion of water, the mixture became very turbid, sometimes clots were formed in a pearl liquid, in which a fine sediment took place, which appeared much more globular than the clots or curdy masses.

4. In general, water in which pus has been agitated remains somewhat milky, with an abundant close white sediment; but after two, or three, or more ablutions, the water becomes clear on standing, and the sediment more curdy.

SECT. IV. *Agency of Alcohol of Wine.*

Action of alcohol on pus.

The different kinds of exsiccated pus exposed to the agency of this menstruum, and treated as described in a former paper, Phil. Trans. 1809, P. II, p. 329*, the results were similar, except in the proportion of products.

1. These exsiccated substances afforded to this menstruum a smaller proportion of potash, but as much animal oxide and muriate of soda, as mucous sputum.

2. The undissolved matter left after repeated digestions in this menstruum afforded the same substances, but in smaller proportions, as mucous sputum.

3. Equal bulks of fresh pus, and rectified spirit of wine, afford a much thicker and more milky liquor, with a closer sediment, than expectorated mucous matter.

SECT. V. *Agency of acetous Acid.*

Action of acetous acid on pus.

The purulent matters mixed with this acid became curdy, and rendered it milky; but on standing, a close white sediment appeared, the liquid above being clear, except in the case of the viscid pus, which exhibited leafy and fibrous masses, as hath been described with mucous sputum.

By repeated digestion of the different kinds of pus in this menstruum, I obtained the same results, except the proportions of acetite of potash, and muriate of soda being smaller, as related in a former paper on mucous expectorated matter. Phil. Trans. 1809, P. II, p. 336†.

SECT. VI. *Some Experiments with different Objects, especially to distinguish Pus and Mucus.*

Comparative experiments on pus and mucus, with mineral acids.

1. In the agency of sulphuric, nitric, and muriatic acids, in sufficient quantity to dissolve and decompound the substances under inquiry, I could perceive no important difference between them. The purulent matters indeed required a much greater proportion completely to dissolve them, than the transparent sputum. Also the more opaque and dense the sputum, the greater the resistance to dissolution. Sulphuric acid produced black liquids like those containing charcoal, smelling strongly of muriatic acid; but on dilution with water, they became clear. No precipita-

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* Journal, Vol. XXV, p. 260.

† Journal, Vol. XXV, p 266.

tion occurred on dilution with water, and on saturation with the fixed alkalis, but a trifling sediment appeared, which redissolved on the addition of the above acids.

mineral acids diluted, and vegetable acids,

2. The mineral acids diluted, or added in small proportion, and the vegetable acids, coagulate variously pus and mucous fluids. Some become merely milky fluids, others curdy fluids, others afford fibrous and leafy masses in a transparent liquor, and others give a uniform thick mass of curd. On standing the deposits are accordingly of various forms, and the liquors above of various appearances; but I could discover no constant characteristic property of the substances by these experiments, as some writers have asserted.

fixed alkalis and lime,

3. The solid fixed alkalis, or lime, mixed with expectorated mucus, occasion a stronger smell of ammonia than with pus; or than with muco-purulent sputum. Some use may be perhaps made of this easy experiment to judge of the nature of varieties of the fluids in question, particularly as far as depends on the proportion of ammonia; for sometimes it cannot be perceived by the smell on mixing alkalis, but can by muriatic acid giving white vapours.

strong solutions of alkalis,

Concentrated liquid alkalis, added to both pus and mucus, dissolve them to produce clear liquids, except small curdy parts and motes. These curdy parts and motes resist dissolution also for some time even in nitric acid, and seem to be self-coagulated lymph. They are in much greater proportion in pus than mucus. The addition of acids to these alkaline dissolutions occasions precipitations: but no differences, or not with sufficient uniformity to afford criteria, were observed according to the observations of other experimenters.

strong solutions of neutral salts,

4. Concentrated aqueous solutions of various neutral salts, *viz.* muriate of ammonia; nitrate of potash; muriate of soda; sulphate of soda, &c.; being mixed in due quantity with pus of the kinds under examination, produce viscidity, like ropy expectorated matter, thickening like jelly, and less opacity. These changes have, in the case of muriate of ammonia, been called coagulation by Mr. Hunter; but by agitation in cold water the matters are diffused, and on standing, the pus is precipitated in its original state. I call these effects of the neutral salts inspissation, seemingly occasioned by their attracting water from the pus; for no

such change is produced if either the purulent matter, or solution of salts, be diluted; nor is it produced if the pus be previously coagulated by caloric: also the inspissated pus is coagulable by caloric as usual. No such inspissation is produced by these salts in mucous sputum, or in mucopurulent sputum, so that undoubtedly it is a criterion as discovered by Mr. Hunter in the case of muriate of ammonia, and with other neutral salts, as now manifested.

and other tests.

4. I endeavoured to find some easy tests for distinguishing pus from mucus; but I did not succeed with the tanning principle; gallic acid; supersulphate of alumina; nitrate of silver, and other metallic salts; and as already said, various acids. They all produced precipitation of these animal matters, but not with observable characteristic differences.

State in which pus is secreted.

5. To observe the state in which the matter of pus is secreted, I procured the assistance of Mr. Maynard, the present house-surgeon of St. George's hospital, and Mr. George Ewbank, who had been on many occasions essentially serviceable in my inquiries. Square pieces of goldbeater's skin were applied to various sore legs after carefully removing the matter already secreted. In five or ten minutes the square pieces being removed, they were found wet with a limpid fluid. In this state they were inspected by the microscope, by which numerous globules were seen. In ten minutes farther the liquid was no longer limpid but opaque, like pus, in which the usual spherical particles were seen with the microscope as juſt mentioned.

Supposing objections might be offered on account of the alteration of texture of the skin employed, square pieces of glass were also applied. The results were the same in both trials. The two gentlemen above named, as well as Dr. Richard Harrison, and other pupils, who happened to be present, all concurred in the observation, that the limpid matter became opaque, and that while limpid it was, like pus, full of spherical particles.

(To be concluded in our next.)

V.

An Account of a New Gas, with a Reply to Mr. MURRAY'S *last Observations on Oximuriatic Gas. By Mr.* JOHN DAVY.

To MR. NICHOLSON.

SIR,

Mr Davy's theory opposed by Mr. Murray.

ABOUT six months since Mr. Murray undertook to oppose Mr. Davy's theory respecting oximuriatic gas, and to defend the old hypothesis, in which this substance is considered as a compound of oxigen and an unknown basis called muriatic acid, and common muriatic acid gas as a compound of the same basis and water.

His experiments.

Independent of his general reasoning, the only arguments advanced by this gentleman in support of his opinions were derived from his own experiments, undertaken expressly for the purpose. His first attempt to discover oxigen in oximuriatic gas was by trying the action of this substance on carbonic oxide; and he concluded, that it did not exert any when the mixture of the two gasses, previously dried, was exposed to the influence of light. He then endeavoured to prove, that the addition of hidrogen to the mixture induced action, and the formation of carbonic acid gas. He also attempted to show that oximuriatic gas, if supplied in sufficient quantity, is capable of affording oxigen to sulphur in sulphuretted hidrogen, and of converting it into sulphureous or sulphuric acid.

Objections.

To account for these supposed changes in consequence of the presence of hidrogen, he was obliged to imagine, in opposition to all experimental evidences, that the composition of muriatic acid gas is indefinite: that the unknown basis combines with different proportions of water, but always retains the appearance and the gaseous state of common muriatic acid gas, hitherto the only subject of experiment.

Having given this outline of Mr. Murray's mode of defence of the old hypothesis, I shall briefly state the facts I ventured to oppose to it.

Water present

It was first shown, that muriatic acid gas, and the sulphuretted in the experiment.

phuretted liquor of Dr. Thomson, alone resulted from the action of dry oximuriatic gas on dry sulphuretted hidrogen; and that the production of sulphuric acid in Mr. Murray's experiment was owing to his having admitted water.

New compound.

My brother, Mr. Davy, next discovered the existence of a new gas made in the same way as the gas employed in Mr. Murray's first experiments, in which he says he obtained carbonic acid, and possessed of the property of converting carbonic oxide into carbonic acid, it being a compound of oximuriatic gas and oxigen.

Experiment to show that oximuriatic gas does not acidify carbonic oxide.

Lastly, it appeared, that due allowance being made for the difficulty of entirely excluding moisture, pure oximuriatic gas is not capable of converting carbonic oxide into carbonic acid, when inflamed with a mixture of this gas and hidrogen. Thus, when 10 measures of carbonic oxide were subjected to the action of oximuriatic gas inflamed by an electric spark with hidrogen, only two measures disappeared, 8 measures of carbonic oxide remaining unaltered. A result perfectly satisfactory, I conceived, considering the minute quantity of the gasses operated upon, not altogether amounting to half a cubic inch; and recollecting, that half a grain of water contains sufficient oxigen to convert about four cubic inches of carbonic oxide into carbonic acid.

Mr. Murray of a contrary opinion.

Mr. Murray is of a different opinion. He considers, in his last communication, the disappearance of two measures of carbonic oxide, a demonstration, that oximuriatic gas is a compound of an unknown basis and oxigen. In the same paper, which is published in your Journal for June, he has given an account of the repetition of his experiment on the mixed gasses, employing pure oximuriatic gas; and he has arrived at the conclusion, "that the production of carbonic acid is established beyond the possibility of doubt."

I shall state the manner in which he conducted the experiment, and the evidences which satisfied him of the production of carbonic acid.

His experiment in support of it.

He exposed to light a mixture consisting of one volume of carbonic oxide and of the same quantity of hidrogen with twice that quantity of oximuriatic gas. After 36 hours he added ammoniacal gas to complete saturation, and, finding that most of the carbonic oxide had disappeared, and that one of the ammoniacal salts formed had the property of effervescing with dilute nitric acid; he, without any additional proofs, drew the conclusion just mentioned, "that the production of carbonic acid in this experiment "was established beyond the possibility of doubt."

A new acid gas the cause of his mistake.

I have now to announce the existence of a new acid gas, which operated in Mr. Murray's experiment, without his knowledge of its presence, and was the cause of those phenomena, which he erroneously attributed to the formation of carbonic acid gas.

Mr Murray's experiment repeated.

Repeating this gentleman's experiment on the exposure of the mixture of the three gasses to light, and detecting, after the addition of ammonia, no traces of carbonic oxide; and perceiving, as he stated, an effervescence of the ammoniacal salt formed with nitric acid; I was induced to repeat also his experiment on the exposure of a mixture of carbonic oxide and oximuriatic gas to light without hydrogen. In this instance I obtained the same result, a total condensation by ammonia without the slightest remains of carbonic oxide.

Presence of water suspected but not to be found.

So satisfactory were the details of Mr. Murray's experiment, the result of which was asserted to be, "that dry carbonic oxide gas and oximuriatic gas do not act on each other;" that at first I could hardly believe, but that water was somewhere concealed in the apparatus, and I gave myself much trouble to discover its source, but in vain.

The gas examined.

The next step I took was to examine the gas, that resulted from the now evident action of oximuriatic gas on carbonic oxide. Mr. Brande was present at the time.

Its properties.

Finding that it did not fume when thrown into the atmosphere, that it had a most intolerable suffocating odour, that it was colourless, that it did not act on the mercury, and that water absorbed it very slowly, we immediately perceived, that it was a new and peculiar compound of carbonic oxide and oximuriatic gas, and this conclusion is fully confirmed by the investigation I have made of its properties.

I shall now mention only the most striking circumstances respecting it. It is my intention to give a full account of the experiments I have made on it, in a paper which I shall soon do myself the honour of offering to the Royal Society.

Its production.

I have found, that it is produced in two or three minutes when a mixture of equal volumes of carbonic oxide and oximuriatic gas is exposed in a tube over dry mercury to bright sunshine; and that the condensation, that takes place in their union, is exactly equal to one volume, so that this is the heaviest gas known excepting silicated fluoric acid gas. I have also ascertained, that it may be at any time formed without the direct rays of the sun—Light alone being necessary.

Its characters as an acid.

Its acid character is well defined. It reddens litmus and combines with ammonia; and its saturating power is so great, that it condenses four times its volume of this gas, forming a perfectly neutral salt, deliquescent, and of course very soluble in water; and its attraction for the dry volatile alkali is so strong, that it decomposes carbonate of ammonia, and is not expelled by acetic acid from this alkali.

Decomposition of its ammoniacal salt.

The decomposition of this ammoniacal salt with effervescence by dilute nitric acid deceived Mr. Murray. Water in this instance is decomposed, its hidrogen is abstracted by the oximuriatic acid to form muriatic acid, and its oxigen by the carbonic oxide to produce carbonic acid, which is disengaged.

Other properties of it.

This will appear evident, when it is known, that the new gas neither inflames on the passage of the electric spark with either oxigen or hidrogen alone, but that it detonates violently with a mixture of oxigen and hidrogen in proper proportions, and affords only muriatic and carbonic acid gas.

Action of metals on it.

The action too of several metals and their oxides on this gas is perfectly consistent with, indeed is quite demonstrative of its being a compound of equal volumes of carbonic oxide and oximuriatic gas, so condensed as to occupy half the space of the mixture of the two. Thus tin, zinc, and antimony, respectively heated in it in small bent glass tubes over mercury rapidly decompose it. In each instance carbonic oxide, exactly equal to the volume of the gas decomposed, is liberated, and a compound of the metal employed and oximuriatic gas is produced, the same precisely as is formed by the combustion of the metal in oximuriatic gas. The decomposition too is just as readily effected by the oxides of zinc and antimony; with the first carbonic acid gas is obtained, and a compound of zinc and oximuriatic gas; but with the last, the fusible protoxide being used, butter of antimony is produced, and carbonic oxide liberated, and an infusible peroxide formed, a proof, if any was required, of the formation of carbonic acid in the preceding instance being owing to the decomposition of the oxide of zinc, and not of the oximuriatic gas.

It is composed of two acidifying principles united to one base.

These are some of the principal circumstances I have discovered respecting this new gas; a gas, which, as it reddens litmus and expels acids from ammonia in consequence of superior-attraction, has every claim to be considered as a peculiar acid singularly composed of two acidifying principles united to one inflammable base.

After the preceding statement of facts, Mr. Murray, I should conceive, will be induced to renounce his conclusion, "that the production of carbonic acid in his experiment was established beyond the possibility of doubt;" and admit, that what he considered as carbonic acid was actually the new gas just described; and I should likewise imagine, that this gentleman in future will be more cautious in his assertions, and criticisms on the labours of others. Let the intelligent candid reader judge of the propriety of the following observation. Mr. Murray says, having previously stated, that he had found carbonic acid in all his experiments, "that the Messrs. Davys did not obtain it in theirs, because they did not look for it with sufficient care, or were not sufficiently aware of the fallacies, by which its production might be concealed." His considering the new gas as carbonic acid is another instance of the evil tendency of attachment to hypothesis. How just is the remark of Lord Bacon! quod mavult homo esse verum, id facile credit.

No carbonic acid formed from the combustion of dry carburetted hidrogen and oximuriatic gas.

In a former communication I have observed, that no carbonic acid appeared to be formed, when dry carburetted hidrogen and oximuriatic gas are inflamed by the electric spark, assigning as a reason for this belief, the precipitation of charcoal. I tried both olefiant gas and carburetted hidrogen procured by the decomposition of acetate of potash by heat. Mr. Murray says, that he has repeated the experiment, and that in this too I was deceived. Mr. Murray employed the gas produced by heat from moistened charcoal. It is surprising that he is not aware, that Dr. Henry found this gas to be a mixture of carburetted hidrogen and carbonic oxide; and that the formation of carbonic acid might be expected on passing a mixture of it and oximuriatic gas frequently through lime water, as he experienced, this being the result when a mixture of pure carbonic oxide and oximuriatic gas is thus treated.

There is nothing farther in Mr. Murray's communications, that requires notice, excepting a misuse of names. He sometimes writes properly, calling me "Mr. J. Davy," at other times, improperly "Mr. Davy," thus creating confusion, and rendering it impossible to distinguish opinions and statements which belong to me, and for which I alone am answerable, from those of Mr. Davy, my brother.

I am, Sir,
Your obedient humble servant,
London, August the 9th, JOHN DAVY.
1811.

VI.

Method of preparing a beautiful and permanent White for Water Colours. In a Letter from Mr. GROVER KEMP.

To MR. NICHOLSON.

SIR,

It is our duty to communicate all we can to the public.

IT is a very just remark of the ingenious and candid Chaptal, that, "at a time when the minds of all men are bent on confirming public happiness, every citizen owes to his country all the services that his situation allows him to accomplish; he should be eager to pay to society the tribute of those talents with which Heaven has favoured him; and there is no one who is not able to bring some materials to the foot of that superb edifice, which a virtuous government is erecting to the happiness of all[*]:" and believing with this

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[*] See "Elémens de Chimie par M. Chaptal," Montpellier edition, 1790. Advertisement, p. 5.

A permanent white for water colours.

celebrated chemist, that it is the duty of every one to do what he can towards the advancement of general science; I am induced to lay before the public, through the highly respectable medium of the Philosophical Journal, a new and easy method of preparing a beautiful permanent white for water colours, calculated to stand the test of time; which, I understand, is at present a great desideratum among our artists. This being the case, I entertain a confident hope, that the present discovery may prove eminently useful.

Mentioned by Mr. Hume.

Through the information of a chemist of the name of Hume, the public is already in possession of the facts, that a colour can be prepared from barytes; and that this earth will furnish the only white for water painting, that never changes; which may also be mixed with any other colour without injury; but of its mode of preparation we have hitherto, I believe, remained entirely ignorant. This beautiful pigment, which not only surpasses in opacity and whiteness every thing of the kind I have ever met with, but possesses the peculiar advantage of being permanent, is prepared by the following simple process: Dissolve pure barytes, or the common native carbonate, in diluted nitro-muriatic acid; filter the solution, and add thereto as much carbonate of ammonia, previously dissolved in distilled water, as is sufficient to precipitate the earth; which may be separated by filtration, and, after repeated washings with distilled water, must be gradually dried by the heat of the sun, or a fire, and rubbed into a very fine powder, or made up into cakes for use. I decomposed some nitro-muriate of barytes with a solution of pure ammonia, but the precipitate was very inferior in colour to the above. An artist of acknowledged celebrity, who has used this white, speaks very encouragingly of it.

I remain, respectfully,

GROVER KEMP.

Brighton, 8th mo. 4th,
1811.

VII.

VII.

The Natural History of Clouds. By LUKE HOWARD, *Esq.*[*]

A CLOUD is a visible aggregate of minute drops of water suspended in the atmosphere.

Etymology and definition of the term cloud.

The word is probably derived from the Anglo-Saxon ceh-loð, *covered*, *hidden*, the face of heaven being so in those parts where clouds appear. The same aggregate, which in this situation is called cloud, obtains the name of mist, when seen to arise from the earth or waters; and fog, when it envelopes and covers the observer. Yet the two latter, viewed from a greater distance or elevation, present all the appearances of clouds; while these, in their turn, become mists and fogs, in proportion as we approach and penetrate them. It may be proper, therefore, for the sake of precision, that the term cloud, in philosophical language, should be made a general one, comprehending all such aggregates, however situate.

Formed of drops of water.

It is concluded, from numerous observations, that the particles of which a cloud consists are always more or less electrified. The hypothesis, which assumes the existence of vesicular vapour, and makes the particles of clouds to be hollow spheres, which unite and descend in rain when ruptured, however sanctioned by the authority of several eminent philosophers, does not seem necessary to the science of meteorology in its present state; it being evident, that the buoyancy of the particles is not more perfect than it ought to be, if we regard them as mere drops of water. In fact they always descend, and the water is elevated again only by being converted into invisible vapour.

Natural History of Clouds.

Prognostications of weather from them.

Since the general introduction of accurate instruments for determining the changes of density, temperature, humidity, and electricity, which continually occur in the atmosphere, our knowledge of its constitution and properties has been considerably advanced. It is nevertheless true, that the philosopher of the present day is not more weather-wise than his predecessors in ancient times. He is still obliged to yield the palm in the science of prognostics to the shepherd, the ploughman, or the mariner; who, without troubling his head about the reasons of things, has learned, by tradition and experience, to connect certain appearances of the sky with certain approaching changes; of which those appearances are, in fact, a commencement or continuation, discoverable while the cause is yet at a distance. Undoubtedly the union of these two kinds of knowledge would best deserve to be entitled the science of meteorology; and it must tend, equally with the invention or perfection of philosophical instruments, to the improvement of this science, could we restore to its place the ancient and popular branch of it, now too much neglected by philosophers, which is founded wholly on natural phenomena. If we except the changes of the wind, some indications of moisture and dryness, and a few others of less importance, the whole of these may be traced to one common origin in the product resulting from the decomposition of vapour; which remains, during a certain interval, in a state of simple diffusion or suspension in the atmosphere. To give to the extensive collection of facts, which it is easy to make on this subject, a communicable and useful form; to render that attainable in a short time, which has been hitherto the exclusive treasure of the adepts of long experience, is the object of the writer of the following systematic nomenclature and natural history of clouds.

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[*] This valuable paper was first inserted in Mr. Tilloch's Philosophical Magazine, and reprinted, with the author's revisions, in Dr. Rees's New Cyclopedia, article CLOUD, from which I have copied it, in order that the readers of our Journal may more completely understand the Meteorological Tables, which will in future appear in our work. W. N.

Modifications of Clouds.

Clouds are susceptible of various modifications.

By this term is intended the structure or manner of aggregation, in which the influence of certain constant laws is sufficiently evident amidst the infinite less diversities resulting from occasional causes.

Hence the principal modifications are as distinguishable from each other, as a tree from a hill, or the latter from a lake; although clouds, in the same modification, compared with each other, have often only the common resemblances which exist among trees, hills, and lakes, taken generally.

There are three simple and distinct modifications, which are thus named and defined.

1. Cirrus. *Def.* Nubes cirriformis tenuissima, quæ undique crescat. Cirrus.

The Cirrus. A cloud resembling a lock of hair, or a feather. Parallel flexuous, or diverging fibres, unlimited in the direction of their increase.

2. Cumulus. *Def.* Nubes densa cumulata, sursum crescens. Cumulus.

The Cumulus. A cloud which increases from above in dense, convex, or conical heaps.

3. Stratus. *Def.* Nubes strata, aquæ modo expansa, deorsum crescens. Stratus.

The Stratus. An extended, continuous, level sheet of cloud, increasing from beneath.

There are two modifications, which appear to be of an intermediate nature; these are:

4. Cirro-cumulus. *Def.* Nubeculæ subrotundæ connexæ vel ordinatè positæ. Cirro-cumulus.

The Cirro-Cumulus. A connected system of small roundish clouds, placed in close order, or contact.

5. Cirro-stratus. *Def.* Nubes extenuata, sub-concava vel undulata. Nubeculæ hujusmodi appositæ. Cirro-stratus.

The Cirro-stratus. A horizontal or slightly inclined sheet, attenuated at its circumference, concave downward, or undulated. Groups or patches having these characters.

Lastly, there are two modifications, which exhibit a compound structure, *viz.*

6. Cumulo-stratus. *Def.* Nubes densa, quæ basi cumuli structuram patentem cirro-strati, vel cirro-cumuli superdat. Cumulo-stratus.

The Cumulo-stratus. A cloud in which the structure of the cumulus is mixed with that of the cirro-stratus, or cirro-cumulus. The cumulus flattened at top, and overhanging its base.

7. Nimbus. *Def.* Nubes densa, supra patens et cirriformis, infra in pluviam abiens. Nimbus.

The Nimbus. A dense cloud, spreading out into a crown of cirrus, and passing beneath into a shower.

Of the Cirrus.

Cirrus described.

Its formation.

ery lofty.

This is always the least dense, and commonly the most elevated modification. It is sometimes spread horizontally through a vast extent of atmosphere; the whole breadth of the sky being insufficient to show where it terminates. In this case, its parallel bars appear, by an optical deception, to converge in opposite points of the horizon. At others, it is exhibited in unconnected perpendicular bundles, of the most minute size. Between these extremes, it may be traced in every degree of extent and inclination to the horizon. In a serene sky the cirrus is first indicated by a few threads, pencilled in white, on the azure ground. Its increase takes place in various ways, and may be compared sometimes to vegetation, more often to crystallization. Thus, 1. Parallel threads are added to each other horizontally, and occasionally other strata of the same, crossing the first at right or oblique angles, until a delicate transparent veil is formed. 2. Parallel threads are collected into distinct groups, lying at various angles with the horizon. 3. Flexuous and diverging fibres are extended from the original stem, forming the resemblance of crests of feathers, locks of hair, &c. 4. The first formed threads, become, as it were, the supports from which others obliquely ascend or descend into the atmosphere. Lastly, A dense nucleus is sometimes formed, and short fibres shoot out from it in all directions. The great elevation of the cirrus has been ascertained by geometrical observations. "The small white streaks of condensed vapour, which appear on the face of the sky, I have found," says Dalton, "by several careful observations, to be from three to five miles above the Earth's surface."

Viewed from the summits of the highest mountains, they appear as distant as from the plains. A more easy and not less convincing proof of their elevation may be deduced from their continuing to be tinged by the sun's rays in the evening twilight with the more vivid colours of the prism, while the denser clouds, having already passed through the same gradation, are in the deepest shade.

The

Its duration.

The duration of this cloud varies according to its staton in the atmosphere, and the presence or absence of other clouds: it is long, extending sometimes to thirty-six hours, when it appears alone, and at its greatest elevation; but shorter, or even very transient, when formed lower, and in the vicinity of the cumulus.

Its motion.

Its connexion with the wind.

By an inexperienced observer the cirrus would be pronounced absolutely motionless. On comparison with a fixed object, however, it is sometimes found to have a considerable progressive motion. The propagation of the cirrus, and the variable directions of its flexures, merit attentive observation, as being intimately connected with the variations of the wind, although undoubtedly not produced by the mere motion of the air.

The general principles, which the imperfect notice hitherto bestowed on it seems to point out, are the following:

Indications from it.

1. Its appearance is a general indication of wind; and it is most conspicuous and abundant before storms.

2. It is often a leeward cloud; or, when a group of cirri appears on the horizon, it seems to invite a current towards it: and the wind very often shifts into that quarter towards which the points are directed.

3. Horizontal sheets of the cirrus, more particularly those which carry streamers pointing upward, are among the indications of rain approaching, while the fringe-like depending ones are found to precede fair weather.

Of the Cumulus.

Cumulus described.

Its formation.

Clouds in this modification are commonly of dense structure. They are formed in the lower atmosphere; and move with the wind, or more properly with that current which flows next the Earth. The phenomena of the cumulus are usually these: In the latter part of a clear morning, a small irregular spot appears suddenly at a moderate elevation. This is the *nucleus*, or commencement of the cloud, the upper part of which soon becomes convex and well defined, while the lower continues irregularly plane. On the convex surface the increase visibly takes place, one heap or protuberance succeeding another, and again losing itself in a subsequent one, until a pile of cloud of an irregular hemispherical form is raised; which floats along, presenting its apex to the zenith, while the base, or rather the lower surface of the baseless fabric, continues parallel to the horizon.

When these clouds are of considerable magnitude, they remain at proportionably great distances. When smaller, they croud the sky by a nearer approach to each other. In each case the bases range in the same plane; and the increase of each keeps pace with that of its neighbour, the intervening space remaining clear.

Decrease.

The cumulus often arrives at its greatest magnitude early in the afternoon, when the temperature of the day is at its maximum. As the sun declines, it gradually decreases, retaining its character till towards sun-set, when it is more or less hastily broken up, and evaporates, leaving the sky clear, as in the early part of the morning. Its tints are often vivid, and pass through the most pleasing gradation during this last hour of its existence.

Indication.

The preceding phenomena form the history of the pure cumulus, as it may be termed, when no other modification appears along with it. They are both the accompaniments and prognostics of the fairest weather.

Of the Stratus.

Stratus described.

The stratus has a moderate degree of density. It is the lowest of the modifications, being formed in contact with the earth or water. It comprehends those level creeping mists, which, in calm evenings, spread like an inundation from the valleys, lakes, and rivers, to the higher ground.

Unlike the cumulus, which belongs to the day, and rarely survives the setting sun, this cloud accompanies the shades of night, and commonly vanishes before the ascending luminary. The evaporation commences from below. At the moment of the separation of the stratus from the Earth, its character is changed, and it puts on the appearance of the nascent cumulus.

Indication.

The nocturnal visits of the stratus have been always held a presage of fair weather. Thus Virgil:

> "At nebulæ magis ima petunt, campoque recumbunt."
> Then mists the hills forsake and shroud the plain.

The meteorological axioms of this great poet were probably selected from the popular ones of his age, as confirmed by his own experience. Hence they ever agree with that of his readers. There are few days in the whole year more calm and serene than those the morning of which break out through the stratus. They are the halcyon days of our autumn: an interval of repose between the equinoctial gales and the storms of winter.

Of the Cirro-cumulus.

Cirro-cumulus described.

The intermediate nature of this cloud may be ascertained by tracing its origin, as well as inferred from its structure. The cirrus, in its slow descent through the air, may be seen to pass into this and the next modification; although its previous appearance does not seem absolutely necessary to the production of either.

Most of our readers will recollect the appearance of the icy efflorescences on the panes of windows, gradually melting into an assemblage of drops, which adhere to the glass, retaining somewhat of the same figure, deprived of its right lines and angles. Such is the change of form which the cirrus undergoes, in passing to the state of the cirro-cumulus. And, as the water on the windows is occasionally converted again into spiculæ of ice, so these small rounded masses sometimes suddenly resume the forms of the cirrus. In the oblique denser tufts of the latter, the change to the spheroidal form often begins at one extremity, and proceeds gradually to the other, during which the cloud resembles a ball of flax, with an end left unwound and flying out. All the cirri in the same group, and frequently all those in view, observe the same law in these changes.

Indications.

The cirro-cumulus forms a very beautiful sky. Numerous distinct beds are sometimes seen floating at different altitudes, which appear to consist of smaller and still smaller clouds, as the eye traces them into the blue expanse. It is most frequent in summer; is the natural harbinger of increased temperature; and, consequently, one of the best indications of fair weather, when permanent or frequently repeated. A more transient display of it is, however, frequent in the interval of warm showers, and in winter. There are also certain forms of it, more deep and dense than ordinary, and arranged on a curved base, which enter into the peculiar features of thunder-storm.

It is usually found to accord with a rising barometer.

Of the Cirro-stratus.

Cirro stratus described.

This is a multiform cloud, and can only be detected in its various appearances by an attention to its distinctive characters. It is always an attenuated sheet, or patch, floating on the air, in a position nearly or quite horizontal. As we have compared the cirrus to dry flax, we may here consider it as drenched in water, and having its spreading fibres reduced to a closer and recumbent form. Viewed over head, it is remarkable for its uniform hazy continuity, and in the horizon for its great appearance of density, the consequence of its being seen edgewise. In this situation, also, it sometimes cuts the sun's or moon's disk across with a dark line; of which Virgil,

Indications.

> " Ille ubi nascentem maculis variaverit ortum
> Conditus in nubem, medioque refugerit orbe,
> Suspecti tibi sint imbres; namque urget ab alto
> Arboribusque, satisque notus, pecorique sinister."
>
> Georgic, lib. i.

> Or should his rising orb distorted shine
> Through spots, or fast behind a cloud's dark line
> Retire eclipsed; then let the swain prepare
> For rainy torrents: a tempestuous air,
> Swift from the southern deep, comes fraught with ill,
> The corn and fruits to waste, the flocks to chill.

The cirro-stratus is the natural indication of depression of temperature, wind, and rain. In order to make a proper use of it in this respect, it is necessary to attend to the time of its appearance, to its continuance, and its accompaniments. This cloud sometimes alternates with the cirro-cumulus, either at different intervals of the day, or in the same sky, or even in the same stratum, which may consequently be seen successively in each modification, and at intervals, partly in one, partly in the other. In this case the prognostic is doubtful, and regard is to be had to that which ultimately prevails.

Again, there is a transient appearance of the cirro-stratus; which often accompanies the production of dew in the evening, and denotes an atmosphere but lightly surcharged with vapour. Not so when it appears earlier in the day, or at sun-rise (according to the preceding quotation), and attended with the rudiments of the cumulus. In general, the weather may be suspected of a strong tendency to wind and rain, as often as the sky is both hazy, and deformed with numerous small patches of cloud, in which the extenuated character predominates; and these appearances, together with an abundance of cirro-cumulus, indicate thunder. Before storms of wind, there is in particular a feature of cirro-stratus, often very slightly expressed, and in one quarter only, which resembles the architectural cyma.

Haloes, parhelia, &c.

But the most formidable appearance of the cirro-stratus is that of extensive sheets, descending from the highest regions of the atmosphere, and scarcely discernible for a time, but by the prismatic colours which they assume in the vicinity of the sun's or moon's place. These are the skreens on which are described the immense circles of haloes, forming, by their occasional intersections, parhelia, and paraselenia, mock suns and moons, which sometimes vie in splendour with the luminaries themselves. It is easy for those who are acquainted with the principles of optics, to conceive how these intersecting circles are produced by light passing through sheets of cloud placed at different heights and angles.

Consistent with this is the prognostic of foul weather commonly deduced from the appearance of the halo. After a solar halo in spring, or the early part of summer, a series of wet and cold weather may be expected, although it should not commence for some days; during which, nevertheless, the same state of the atmosphere subsists, as is often manifest from the repetition of the halo. Those which surround the moon in clear nights indicate rain or snow, according to the season of the year.

In mountainous and even hilly countries, the cirro-stratus is frequently seen adhering to the more elevated points of land. In winter it also visits the plains, in the form of a very wet and durable mist, the drops of which are nevertheless too small to be visible, and which, unlike the stratus, is more dense on rising grounds than in the valleys.

The cirro-stratus usually accords with a sinking state of the barometer.

Of the Cumulo-stratus.

Cumulo-stratus and its formation described.

The formation of the cirro-cumulus, or cirro-stratus, by condensed vapour, descending from the higher atmosphere, does not prevent the cumulus from being produced out of the water, which, in the mean time, evaporates from the Earth, and ascends to the middle region. In this case, the two modifications after a while come into contact, and present to the attentive observer a succession of curious appearances.

While the cumulus is rapidly increasing upward, a delicate fleece, of a structure visibly different, sometimes attaches itself to its summit, where it reposes as on a mountain. This fleece is a cirro-stratus; and the materials of which it is formed are brought by a superior current overtaking or meeting the cumulus. Frequently, the cumulus in its increase breaks through the cirro-stratus, and appears again above it, but with a visible change in the aggregation, which now becomes rocky, perpendicular, and, finally, overhanging. If the cirro-stratus ſhould itself increase too fast to be swallowed up by the cumulus, the latter after a while extends its protuberances laterally, and attaches itself by them to the superior mass of cloud.

When the cirro-cumulus, in like manner, occupies the superior place, a cumulus rising beneath it is susceptible of the same union by mutual attraction; the result of which, as in the former case, is a large, lofty, and dense cloud, which often subsists through the day; and in the evening undergoes the usual evaporation.

It is not, however, absolutely necessary to the production of this cloud, that either of the superior modifications should be previously formed. In a favourable state of the atmosphere, the cumulus itself, after having arrived at a certain magnitude, suddenly begins to overgrow its base, and produces a cloud, which, in regard to both its form and its rapid growth, may be compared to a mushroom.

The cumulo-stratus usually prevails in the completely overcast sky. In this it presents appearances not easy to be described, but which may be classed by a due attention to the theory of this cloud. At present it is intended to comprehend under it every mode of union between different ſtrata, which is not productive of rain. Future investigation may point out distinctions, which at present we are not prepared to make.

Indications.

This modification is most frequent during a mean elevation of the barometer, or that which is denominated *changeable*, when the wind blows from the west, with occasional deviations towards the north and south. In respect to temperature, it has a wide range, and may usher in a fall of snow, as well as a thunder-storm. Of the latter, indeed, it is among the regular harbingers, but with peculiar appearances. During the suffocating calm which prevails before the first discharge of the atmospheric electricity, it may be seen in different points of the horizon, rapidly swelling to a stupendous magnitude, most curiously wreathed and curled, "fretted and embossed" in its substance, and flanked at different heights by the delicate opake streaks of the cirro-stratus. The whole presents a spectacle of peculiar magnificence, in contemplating which one may imagine an invisible agent collecting in this immense laboratory the energies of the storm, and arranging innumerable batteries for the subsequent explosions.

It will appear by what we have already stated, that the cumulo-stratus affords in general a doubtful prognostic. When it is formed in the morning, the day often proves fair, though overcast; and if the cirro-stratus has contributed to its formation, there will probably ensue heavy showers on the second or third day. When it subsists a long time, the character of its superior spreading part may be consulted, which, if it be decidedly either that of the cirro-stratus, or cirro-cumulus, the usual result of their appearance may be expected.

Of the Nimbus.

Nimbus de-

To have a correct notion of this cloud, the reader has only to scribed.

to take the opportunity of examining a shower in profile as it approaches from the horizon. He will see the dense gloom, which experience teaches him to regard as a mass of descending rain, losing itself above in a cloud, which commonly spreads in one continuous sheet to a great distance all around the shower; insomuch that while the latter is on the horizon at several miles distance, the edge of the cloud has frequently arrived in the zenith. He will perceive, that this spreading crown of the shower advances regularly before it, and that, whether viewed from a distance or over-head, it exhibits in a greater or less degree the fibrous structure of the cirrus. After the shower has passed over, he will commonly observe the same appearances in the part of the cloud which follows it; and in squally weather he will sometimes be able to repeat these observations on many different showers appearing successively; or at the same time, in different quarters. The term *nimbus* is intended strictly to denote no more than this inverted cone of cloud, from which a sudden or dense local shower, whether of rain, snow, or hail, for the difference is not essential in either case, is seen to descend. As it rises to a great height in the atmosphere, it may be seen from a distance of many miles; and so constant is the result of a shower arriving with it, that though, in a few instances, perhaps from the small quantity of the rain, we have not been able to discover the usual obscurity beneath it, while at a distance, we believe it may be laid down as a general rule, on as good grounds as in most other cases, that rain, snow, or hail, is falling on the tract over which it is spread.

> "Qualis ubi ad terras abrupto sidere *nimbus*
> It mare per medium, miseris heu prescia longé
> Horrescunt corda agricolis." Virgil.
>
> So while far off at sea the storm-cloud lowers,
> And on the darken'd wave its fury pours,
> Mid crops unreap'd the hapless peasants stand,
> And shuddering view its rapid course to land.

There is a great difference, at different times, in the proportion which the inverted cone of cloud bears to the column of rain, &c., in which it terminates; and in a very turbid and moist atmosphere, the character of the upper part often approaches more nearly to the cirro-stratus than the cirrus; The more perfectly distinct and local the shower, and the clearer the rest of the air from other clouds, the more perfect the crown of cirrus, which, indeed, sometimes assumes an almost geometrical precision in its form and internal structure; the threads of the cirrus tending from all sides directly towards the top of the column.

Increased by cumuli:

The pure nimbus commonly moves with the wind, and from the rapidity of its passage affords but little to the rain-gauge. But it often happens, that it is formed in the midst of cumuli, which have already arrived at a great size. In this case the latter may be seen to enter successively into the focus at the top of the column, whence they never emerge; being visibly converted to the purpose of supplying materials for the irrigation, which thus becomes more abundant; and the shower is also occasionally thus propagated in a direction opposite to the wind.

Changes:

The nimbus, moreover, does not always originate in a cirrus. The cumulus, and more often the cumulo-stratus, may be seen to expand at their summit into a cirrose sheet, while the lower part is resolved into rain. On the contrary, the rain suddenly ceasing, and the nimbus remaining entire, the sharp extremities of the crown often retire into it; the sides assume the swelling folds, and the character is exchanged for that of cumulo-stratus. When the shower has expanded itself, and the sheets break, the superior portions usually turn to the cirro-cumulus or cirro-stratus, and the lower to the cumulus. When a total evaporation of the remaining cloud follows a shower, it is a very favourable prognostic. A nimbus is frequently accompanied by a cirro-stratus or two lying near it, and on a level with the densest part of the cloud. The nimbus of thunder-storms has many of these, as before observed of the cumulo-stratus, arranged at different heights; which, with the grotesque form of each cloud, and the hazy state of the medium, are sufficiently characteristic of the high electric state of the air at such times, and want only an attentive perusal (in nature) to enable the observer to ascertain it on future occasions. It appears, that the cumulo-stratus passes to the nimbus by a

Indications.

sudden change in its electricity: for in tracing the progress of a thunder-storm, through a long range of these clouds in the horizon, we have been satisfied, that the clouds, which had ceased to afford explosive discharges, had undergone this change in their superior part, and were pouring down rain; while others, among which the lightning still played, or which were situate beyond it, retained their swelling and rounded forms some time longer.

Of the Origin, Suspension, and Destruction of Clouds.

Origin of clouds.

These aggregates consist of water, raised by evaporation, and become visible by condensation in the atmosphere.

Evaporation.

Respecting evaporation, and the state in which vapour subsists, there has been much diversity of opinion: and, of the several theories proposed, there is not one comprehensive enough to merit exclusive adoption. A number of general principles, however, have been established; which we shall employ, with the aid of those of electricity (hitherto not enough considered in its silent and gradual effects), to explain, though in an imperfect manner, the principal phenomena of clouds.

Vapour.

Evaporation consists in the union of water with caloric, and the escape of the compound as an invisible fluid, which we shall exclusively denominate *vapour.*

The air has no solvent action on it.

The solvent action of the air, to which this effect has been attributed by chemical philosophers in general, has been proved by comparative experiments on the force of vapour in air, and with air excluded, to have no perceptible share in it. The laws which govern the natural process, for these alone here interest us, may be thus briefly stated.

Laws of the natural process.

The force by which water is converted into vapour is directly as its temperature, other things being equal: but this force has to overcome an opposing one, of the same nature, inherent in the vapour which already exists in the atmosphere. For such vapour, by its elastic property, tends to exclude from the space it occupies every additional portion; and consequently to prevent the escape from the water of new vapour. Hence the temperatures being equal, the quantity of vapour produced will be less, the greater the quantity already diffused in the air.

Mechanical effect of air on evaporation.

But, though the *chemical* action of air is imperceptible, its *mechanical* effect is great. A moving atmosphere may double or triple the rate of evaporation, according to its velocity. For not only is the surface, from which only the vapour escapes, thus enlarged and changed; but the nascent vapour itself, which would otherwise hover a while upon it, to the obstruction of the process, is immediately brushed away and diffused.

Explanation of various phenomena.

By applying these principles, we may explain to ourselves various natural phenomena: as for instance; why the wind, after rain, becomes colder than even the rain which fell; being robbed of its caloric by the evaporation of the floating and deposited water, with which it is in contact: why snow sometimes totally disappears without melting, and the surface of ice becomes sensibly wasted and channelled; for these are warm, compared with the dry and frosty air which blows at such times, and consequently evaporate freely. In what manner, again, a strong westerly wind in summer or autumn brings up clouds, which on its cessation descend in rain: for it promotes evaporation by its mechanical effect, and the vapour escapes into an atmosphere already too moist to carry it off to any great distance. This will be evident by recurring to the principle before stated, that the vapour escapes by the force of the temperature of the water out of which it is formed; and, consequently, into a colder atmosphere it will still escape, though continually decomposed thereby.

Decomposition of vapour.

Vapour is decomposed by air, in consequence of the superior affinity of the latter to caloric. This happens in two ways. 1. When vapour escapes or is propelled into air colder than itself; the result being a local dense cloud. 2. When a mixture of air and vapour is cooled: in which case there ensues a general turbiness, which we shall exclusively denominate *haze*. It is occasioned by minute floating particles of water; the caloric which, united to these, formed transparent vapour, having passed into the air.

Haze.

Out of this haze clouds may be afterwards formed, by simple aggregation, or by electrical attraction. It abounds in the atmosphere during the most part of the year, occupying sometimes the higher, sometimes the lower, part thereof.

The quantity in which it exists may be judged of, at some periods, by the appearance of distant objects seen horizontally: at others, by the degree of intensity of the blue colour of the sky, which becomes paler by it, if indeed the blueness is not wholly due to this part of the medium.

Of the Nature of the Stratus.

Nature of the stratus.

This cloud is an example of the decomposition of vapour thrown into air of a lower temperature. The earth or water on which it reposes is always warmer than the cloud, as is also the clear air above. Thus, in a stratus, formed over a field with ponds, the temperature of the earth just below the turf was 57°; of the water, 59°; of the air, at an elevation of thirty feet, 55°; while that of the cloud, at four feet from the ground, was 49·5°. Hence this cloud preserves a level surface; and hence it uniformly vanishes, or begins to be driven upward, as soon as its temperature becomes equal to that of the earth. It is consequently due to the decomposition (in a small portion of the atmosphere) of the vapour which the earth and water continue to emit, after sunset, by the force of a temperature previously acquired. But the change in the lower air, which gives occasion to this local decomposition, is not so easily to be explained: for it appears that very often, in the evening of a clear day, the decrease of temperature in the atmosphere takes place in the same order in which the increase did in the morning: *viz.* beginning from the surface of the earth and proceeding upward. If the air never became colder, on these occasions, than the contiguous soil, the effect might very well be ascribed to the absorption of a quantity of caloric by the latter. But we see that, in the present instance, it became colder by seven degrees, though vapour was still decomposing: and this in a perfect calm, which, in a great degree, forbids another supposition, of the exchange of a quantity of heated air below, for as much cold air from the higher atmosphere, otherwise this would seem a sufficient account of the matter.

Clouds not so good conductors as supposed.

The electric charge of the stratus, which is always positive, and sometimes highly so, notwithstanding the contact of its lower surface with the earth, seems to prove, that a cloud is not even so good a conductor as has been supposed, and that the fluid, in certain cases, may be very gradually transmitted through it. Positive electricity being that proper to the atmosphere in fair weather, we should naturally expect to find it in this cloud.

It might be worth while to examine the air above, with a view to discover whether there exists in the latter a negative counter-charge. It will appear, from a consideration of the principles before stated, why this cloud is almost peculiar to the autumn. The gradual decline of the sun, at this season, keeps the atmosphere constantly surcharged with vapour, which is ultimately disposed of in rain; and hence follow gales of wind. The stratus, therefore, though an immediate indication and accompaniment of fair weather, affords an unfavourable prognostic in the early part of summer; as it shows that a tendency has already begun to extensive precipitation, at a time when the usual predominant feature is increasing dryness.

Indications.

Of the Nature of the Cumulus.

Nature of the cumulus.

The heating effect of the sun's rays on the atmosphere is greatest near the surface of the Earth, and diminishes gradually in ascending. The diminution proceeds in fair weather at the rate of about one degree for each hundred yards, as appears by observations with the thermometer on stations of known difference in altitude.

This inequality appears to give rise to the cumulus, on the same principles as those of the stratus, but the effects are more complicated. Vapour is generated, as before, at the surface of the Earth, but it is thrown into an atmosphere heated by the sun. Here it maintains its elastic state, and, in proportion to the supply from below, the whole quantity existing in the atmosphere is compelled to rise. In doing this, it changes its climate, and arrives among air of a lower temperature, where a portion is continually decomposed, filling the middle region with haze. Of this, small aggregates begin to be formed, the increase of which is at first determined by no particular law. But the aggregate is not in equilibrium with the air. It tends to subside, and in the

Nature of the cumulus.

mean time the increase of temperature is proceeding upward. Hence the lower part soon finds a position in a plane of air sufficiently warm to evaporate it: and as this effect is regulated, in general, by the elevation alone, we see these aggregates assume each a flat base, resting as it were on the same plane, parallel to the Earth's surface. The remainder of the cloud sports in all the varieties of the spheroid, and more rarely of the cone; according to the course of the showers of minute particles of water, which we may consider (though invisible in their progress) as descending upon it. The vapour generated at the base is, probably, in part condensed on the surface of the colder particles of the cloud above. While the supply from the haze exceeds the waste by evaporation, the cloud increases: when the latter has begun to prevail, it may be traced through various stages of diminution to its final wreck, on sinking wholly into the warmer atmosphere. This happens commonly about sunset; because the ascending current of vapour, the source of the phenomenon, then slackens or ceases; and the lower air parting with its redundant caloric to the higher, we unexpectedly see the dense clouds evaporate, at the very time when the chill of the evening is felt below, and the dew falls.

But it does not appear, that the causes we have hitherto enumerated are fully adequate to the phenomenon. The increase of the cumulus is often more rapid than consists with the notion of simple attraction, exercised between distant particles of water, in a resisting medium. When a cumulus is thus increasing, the small aggregates in its way do not usually join it, but seem to vanish before it. Lastly, the cumulus itself, however dense, never descends in rain. It is difficult to conceive, that so powerful an attraction could exist for many hours, without bringing the particles together into larger and larger drops, until they were too heavy for longer suspension. If we suppose, however, that, from the commencement of its aggregation, the cumulus becomes a positively electrified mass, these difficulties vanish. This mass may electrify negatively, and attract into itself, from great distances, both the dispersed particles of water and those which have already united in much smaller masses.

Its particles must be mutually repulsive, and cannot come into contact without a change of state: the same may be said of the respective clouds in this modification, when they do not differ too much in surface.

Of the Nature of the Cirro-stratus.

Nature of the cirro-stratus.

When a portion of the atmosphere, charged with vapour, is brought over a tract of land of lower temperature than itself, its caloric is abstracted in sufficient quantity, usually to occasion a decomposition of some of the vapour, and a consequent general turbidness.

Dampness on walls &c.

Mist.

The sweating, as it is improperly called, of walls and pavements in a thaw, and when rain is about to come on, is from this cause; the vapour being decomposed on their surfaces. The mist which ensues at these times obscures distant objects, and occasions the trees, against which it is borne by the wind, to drip plentifully. It is in fact a cirro-stratus in contact with the Earth, and no phenomenon is more familiar to the inhabitants of hilly tracts. The same general depression of temperature may happen in another way, and higher in the atmosphere. When a cold and moist air flows over a warmer vaporous one, it is obvious, that the former may be warmed, and become more transparent, at the expense of the latter; which, from the same cause, must become turbid. The haze thus produced will not subside with the uniform motion of dew, but rather in sheets, becoming more dense as they descend, both from the approximation of their particles, and addition from the vapour they meet with. But the cirro-stratus is far from assuming always the simple form, to which the mere effects of gravity might be supposed to give rise. It exhibits changes, which can only be attributed to the acquisition, or passage through it, of such small portions of electricity, as in a humid medium we may conceive a cloud to be susceptible of. On these occasions it tends either to the state of cirrus, or that of cirro-cumulus, of which we shall treat presently.

Indications.

The reason of the prognostic afforded by the cirro-stratus will now be evident. It gives us notice of a change in the state of the superior atmosphere, which we could not otherwise be certain of, until the current, in its course of propagation downward, had begun to affect the denser clouds, thrown up by the superficial evaporation. It is not very uncommon to see the cirro-stratus evidently brought by a wind, moving in a different direction from that wherein the cumuli are immersed on which it settles. In this case the latter are speedily arrested by it, and assume the new course, or descend in rain, by a change of their electricity.

Of the Nature of the Cirro-cumulus.

Nature of the cirro-cumulus.

Let us now reverse the former case, and consider the upper current as both vaporized, and warmer than the air below.

It is probable, that the upper is then cooled by that part of the lower which is next to it, though very slowly, from the difficult transmission of caloric downward. The decomposition of the vapour in the upper current by this means may give origin to the cirro-cumulus; and the peculiar aggregation of this cloud, as distinguishable from that of the cirro-stratus, may be the result of its acquiring electricity in its descent in a much greater degree. Such, at least, is the inference we may deduce from its abundance before thunder storms; when it is occasionally seen to arrive with the wind in extensive flocks or strata, moving with unequal velocity, and by consequence overtaking each other, until they form a dense stationary mass.

Indications.

This explanation of the origin of the cirro-cumulus is principally deduced from an observation, which we have now so often repeated, as to regard it as a meteorological axiom; *that the temperature of the day following, exceeds that of the day on which it appears.* Hence, when it continues to recur daily, the weather still grows warmer, until a thunder-storm, in some quarter of the heated tract, puts a period to the insulation of the clouds.

Of the Nature of the Cumulo-stratus.

In attempting to assign causes to phenomena so complicated, as those which this modification presents, we may be

Nature of the cumulo stratus.

in danger of admitting a greater number than are really necessary. It is apparent, however, that in the state of things most favourable to the production of the cumulo-stratus, there exists a precipitation, independent of that which gives rise to the cumulus, and situate in a higher region. As this precipitation affords sometimes the cirro-cumulus; at others the cirro-stratus, we need not assign to it any other cause than the one already mentioned, *viz.* a superior vaporized current of air. It is not inconsistent with the principles we have laid down respecting the cumulus; that this cloud should also be produced at the same time; it being requisite only that there exist a sufficient action of the sun on the Earth's surface, or a sufficient temperature derived therefrom. The inosculation of these two orders of cloud, the singular union which follows, and the establishment of a new centre of attraction, towards which the whole future increase tends, is the prominent feature in this modification, and the chief fact which remains to be accounted for. As this effect is not constant and uniform, it cannot be ascribed to gravity alone. Reasoning from analogy, rather than from direct experiment, which it is not easy here to apply, we may attribute it to a difference in the electric charge of the respective clouds; which difference, though small, ought to produce the usual appearances of bodies charged plus and minus; *viz.* mutual approach and contact. This effect, however, appears to ensue rather with regard to the masses than to the individual particles.

The effect of the highly vaporized state of the higher atmosphere is often discernible in the cumulus from its earliest appearance; and it is easy to determine, at certain times, that this cloud, if it continue long, will pass to the present modification. The effect we mean to point out is the uneven growth of the cloud; numerous small masses attaching themselves to its surface, and giving it an appearance not unlike the curls of a fleece of wool; particularly when seen beneath the sun, in a situation where the projecting parts may catch the light. If we admit that the cumulus acts, as well by electrical attraction, as by that of gravity, on the surrounding materials, we may here consider them as arriv-

ing

Nature of the cumulo-stratus.

ing by subsidence in too great plenty to be immediately assimilated; in consequence of which they tend to unite among themselves. A still greater quantity of haze in the region next above the cumulus, gives rise to the curious phenomenon of the *cloud-capped* cloud; when the cumulus is covered at its summit with a cirro-stratus; in the same manner as, in mountainous tracts, this cloud reposes on an elevated point of land. The cause is probably alike in each case, whether it be a lower temperature, or a diminished electricity, which determines to this particular spot the commencement of the aggregation of the cirro-stratus. We may next consider the cumulo-stratus perfectly formed, and endeavour to assign a cause for its occasional long continuance: which, however, exceeds the day of its formation only on the approach of thunder: this cloud, as well as the cumulus, very commonly vanishing about sun-set, and reappearing the next day, for some time. The two strata of the atmosphere, which form the superior and inferior boundaries of the cloud, are probably, during this time, in somewhat different states of electricity; the one also depositing water, the other receiving it; the broad surface of the cumulo-stratus may be regarded as a coating applied to the upper stratum; and receiving from it a continual accession of charged particles of water, the electricity of which is slowly transmitted, through the intermediate portion, down to the base of the cloud, which is often some hundred feet below; and where a continual evaporation counteracts the increase above. Here, while the mass continues in this modification, the progress of the electricity downwards is arrested by the dry air: for although the insulated rod is found sometimes to be affected with positive, sometimes with negative signs, while the base of such clouds is over it, this effect is commonly influential; and the rod is not charged, as by the passage of the nimbus. How the electricity of this cloud is affected by the constant evaporation of a portion at the base remains to be ascertained; and the same may be said as to the cumulus.

Of the Nature of the Cirrus.

Nature of the cirrus.

It was necessary to defer the consideration of the nature of this cloud, until we had developed, in a considerable degree, the principles on which our theory proceeds. The reader will have seen, that we assume the fact of the slow transmission of the electric fluid through clouds: which in this, as in a former instance, we apply rather analogically than by induction; the modification in question being usually so high in the atmosphere, that the electric state of the latter, above and below it, cannot easily be found by actual experiment. Proceeding, however, on this assumption, we suppose, that the cirrus resembles in its state a lock of hair, or a feather, insulated and charged; or rather, that its arrangements result from the same cause with those of the coloured powders, which electricians project on a cake of wax, after having touched it with the knob of a charged phial, and which fall into a variety of configurations on the surface. Thus the cirrus may be formed in the air out of such floating particles of water as are present, and may serve the purpose of collecting and transmitting the electric fluid. It is during the prevalence of variable winds, that the cirrus most abounds; and it is reasonable to conclude, that the portions of air, which at these seasons are transported from place to place, gliding over or intersecting each other, usually differ sufficiently in temperature to occasion a slight decomposition of the vapour of one of the currents, and in their electric charge sufficiently to induce a communication by means of the conducting medium so formed. Again, in the gradual cooling of a perfectly calm plate of air, situate at a great elevation, and consequently free from the occasional causes of disturbance which prevail below, it is not improbable that the separation of the caloric from the vapour, and the collection of the electrised water from the air, may go on together, by a process similar to the crystallization of salts, in which much caloric is liberated into the medium. This opinion, at least, seems to be advanced by Kirwan, in his "Essay on the Variations of the Atmosphere," and we may consider the vegetating cirrus as the proper example of it.

Another

Nature of the cirrus.

Another conjecture might yet be started as to the cirrus. It might be regarded as a cloud wholly formed of minute spiculæ of ice; since the air, at a certain elevation, is sufficiently cold throughout the year for this effect. But if it should be found, that the particles of clouds are susceptible of a rectilinear arrangement in any case at a temperature exceeding 32°, there would be no necessity for this supposition.

If the appearances of the cirrus are as frequent and various at sea as on land, it cannot be doubted, that intelligent mariners would find their account in keeping a register of them, as connected with the changes of wind, &c., making due allowance for the change of station in different observations when under sail.

The buoyancy of the cirrus seems to be most perfect during its first increase. It always follows, at length, the common course of gravity; and the change to the cirro-cumulus, or cirro-stratus, which certainly depends on the state of the medium it falls into, may be ascribed to the retention or loss of the electricity.

Of the Nature of the Nimbus.

Nature of the nimbus.

This phenomenon may be thought to be improperly denominated a modification of cloud, since it consists usually of a column of descending rain, snow, or hail, seen in connection with the cloud affording it. As the concluding link in the chain of atmospherical precipitation, it seems, nevertheless, most advantageously placed here; and its history, though far from including all that we may observe, and could wish to have explained, on the subject of rain, is more decidedly illustrative of the nature of clouds in general than that of any other modification. Moreover it is sometimes observed to be formed before the rain begins, which affords sufficient ground for considering it as a distinct modification of cloud. We owe to the bold and penetrating conjecture of Franklin, on the identity of lightning and the electric spark, the invention of a method of investigating the electricity of clouds: which, in the hands of experimentalists, has since brought out a mass of facts abundantly sufficient to establish that proposition; and which also throws considerable light on the theory of rain, and other depositions from the atmosphere. By this method the structure of the nimbus may at any time, when it passes over us, be demonstrated to be that of a natural conductor, by which the positive charge of the higher atmosphere is brought down to the Earth. For this purpose, there is provided a rod of iron, or other metal, well insulated on a pillar of varnished glass, the latter being defended from rain by an inverted funnel, soldered or cemented to the part of the rod next above it. The rod should be furnished with several points of wire, a few inches long; and it need not be an elevated one for this purpose, provided the extremity is clear of other objects capable of drawing off the fluid. The charge is ascertained by pith balls of a larger or smaller diameter, to suit the occasion, suspended by flaxen threads, on a wire fixed into the lower part of the rod, and terminating in a ball. Near the latter it is proper to have another ball fixed on a stout wire, passing into the ground, to which the fluid, when abundant, may escape in sparks. This instrument exhibits a charge of the same kind with that of the air in which it is immersed; or, in case of rain, &c., the charge of the latter, as compared with that of the air. We will give, in the first place, the appearance which we have recently observed during the passage over the rod of a nimbus of the most simple structure, having neither a cumulus nor a cirro-stratus attached to it; which moved along with the lower current through the clear atmosphere, and discharged a shower of large opaque hail, the air below being very dry. During the approach of the cloud from the north-east, the pith-balls remained close until the spreading crown, which characterizes this modification, had arrived in the zenith. At this time, and while the shower itself was still three or four miles distant, they opened negative. As the cloud came nearer, their divergence increased, until it amounted to full two inches, at which time sparks of considerable strength might be drawn from the rod. After this the negative charge gradually went off, and the balls touched again. In a few moments the edge of the shower, mixed with a few drops of rain, arrived at the conductor, and the balls instantly opened positive, the charge gradually increasing

Its electrical state shown.

Phenomena described.

increasing until sparks were emitted more freely than before. This charge continued during the passage of the hail, and went off gradually as soon as it was clear of the instrument. After having closed, the balls opened again negative, and this charge increased to a considerable intensity, as the shower receded towards the south and south-west, after which it gradually went off: the balls closed, and finally were left slightly positive. From these facts, the reader, who is conversant in electricity, will deduce the structure of the lower part at least of the shower. He will see, that the descending hail formed a column positively electrified. This, which might be six or seven miles in diameter, was surrounded with a cylinder of negative electricity, probably extending in every direction three miles farther, and resulting from the action of the positive centre on the dry atmosphere, in which it was moving. Now the amount of the hail, when melted, was considerably less than $\frac{1}{100}$th of an inch in the rain gauge; and could the descent of the electric fluid through the whole space have been rendered as obvious to our senses as that of the hail, we should probably have said, that the shower consisted of fire more truly than of ice.

Whence the electricity?

The question that naturally presents itself is, Whence came this flood of electricity which accompanied the hail? It was not from the circumstance of the water being frozen, since a hard shower of rain equally exhibits a charge, but with this remarkable difference, that whereas snow, sleet, and hail, are always positive, rain is found sometimes positive, sometimes negative. The reader may consult, on this head, an extensive collection of facts in Read's Journal of Atmospherical Electricity, "Phil. Trans." Vol. LXXXII. The probable sources of negative rain will be presently mentioned; but to return to the question of the origin of the positive charge; if we attentively consider the structure of the nimbus, it is precisely that which, from the known properties of the electric fluid, we should propose for a conductor formed to acquire the latter. If we detach from it the falling column, and extraneous clouds which usually attend its progress, it will be found to consist of a close collection of fibres, diverging from the region of the cumulus, (where, it appears, the rapid union of the particles into drops is accomplished,) to a vast height and extent in the superior atmosphere. The conducting line, therefore, may be considered as prolonged from the top of the column to the very extremity of each of these fine fibres of cloud, which are often extended, in all directions, as correctly as those of a lock of hair insulated on a charged conductor. The intention in this case seems to be not so much the precipitation of water, as that of the electric fluid which keeps it in suspension. This purpose accomplished, (and the reader may conceive how great a discharge must be effected by a number of such machines acting at once on a small tract of country,) the water unites into larger drops through the whole extent of the atmosphere; it subsides in a continuous sheet, under which the condensed product of the superficial evaporation moves along, in the form denominated *scud*; and the rain comes down freely and generally, until the atmosphere is disburdened, or until the partial vacuum which is formed brings in a drier air from the northward.

Scud.

Negative or nonelectric rain.

Negative, as well as nonelectric rain (which sometimes falls, though strong positive and negative signs precede or follow it in the clear air) must necessarily result from the action of a central mass of cloud, in which a strong positive charge exists, on the clouds of less extent which fall in its way; and it is to be considered also, that rain, at the elevation in which it is formed, may be perfectly nonelectric, (*i. e.* it may result from the union of clouds differing in electricity, and hence uniting in rain,) yet at the moment of arriving at the Earth it may differ so much in its charge from the atmosphere below, the only standard of comparison, as to be strongly negative or positive with respect to the latter. But these considerations belong more properly to the subject of atmospheric electricity.

Review of the modifications of cloud.

We shall conclude with a brief review of the modifications, ascending from the stratus, formed by the condensation of vapour, on its escape from the surface, to the cumulus, collecting the water arrested in the second stage of the ascent; both probably subsisting by virtue of a positive electricity. From these proceeding, through the partially conducting cumulo-stratus, to the cirro-stratus and cirro-cumulus; the latter positively charged, and considerably retentive of its charge; the former less perfectly insulated, and, perhaps, conducting horizontally; we arrive thus at the region, where the cirrus, light, elevated, and extended, obeys every impulse or invitation of that fluid, which, while it finds a conductor, ever operates in silence; but which, embodied and insulated in a denser collection of watery atoms, sooner or later bursts its barrier, leaps down in lightning, and glides through the nimbus from its elevated station to the Earth.

VIII.

Account of the Thunderstorms on the 19th of August. In a letter from THOMAS FORSTER, *Esq.*

To WM. NICHOLSON, Esq.

SIR,

Thunder-torms on the 9th.

I Wish to communicate to your meteorological readers some observations on the thunderstorms, that happened on the 19th inst., of which I shall request your insertion.

Weather on he 18th.

The 18th was warm, the maximum of the thermometer being about 73°. *Cumuli* prevailed during the day, but towards evening the *cirrus* appeared.

The first storm e cribed.

Before 8 o'clock in the morning of the 19th, the sky was clouded. I observed two *strata*; the upper one appeared to be a uniform veil of cloud, while loose flocky cumuli floated beneath it; and in some places large masses seemed to be attracted towards it, and adhered to its surface, forming an unusual wavy sky, which increased in density. About half after eight I heard a single explosion, like the report of a large brass cannon; about twenty minutes after which two more such reports were heard, following each other in rapid succession, which were immediately succeeded by a long and loud peal of rolling thunder. The storm now came up very fast, in a direction nearly contrary to that of the current of wind below, with hard rain, and thunder and lightning.

Interval.

After the storm had subsided, *cumuli* were again seen sailing under a continuous sheet of cloud; some of them were loose flocculi, others large well defined masses. By degrees they became lost in the upper stratum; the sky became again very black, and thunder and lightning with rain again prevailed. During the process of the storm I heard, (beside the many peals of *rolling thunder*) another loud single explosion, which sounded like the hollow report of a mortar; it was preceded by a very vivid flash of lightning. I dwell particularly on this circumstance, because I have often noticed during storms two very dissimilar kinds of thunder. One is a long roll increasing in loudness while it continues; this is supposed by Mr. B. P. Van Mons to be caused by combustion of the two gasses of water*. The other is a loud and sharp explosion of short duration, and often a single report like that of a cannon; the lightning which precedes this is generally vivid and mischievous, it darts directly towards the Earth, or any other prominent object, as high trees, towers, &c., and is considered by Mr. B. P. Van Mons, as the flying off of electricity from an overcharged cloud†. I wish to direct the attention of meteorologists to the solution of this question. When mischief is done by lightning, is not the thunder which follows the flash generally of this latter kind?

Second storm.

Two dissimilar kinds of thunder.

The variations in the direction of the wind below, in stormy weather, as well as the contrary directions of the current above, constitute another curious object of philosophical speculation. Small air balloons might, in this case, become useful meteorological instruments. I have sent up a great many of them, and have generally seen them moved by several different currents of air.

Various currents of air in stormy weather.

Small air balloons as a meteorological instrument.

Yours &c.

THOMAS FORSTER.

Clapton, Hackney,
22d Aug. 1811.

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* See Journal for October, 1809, Vol. XXIV, p. 106.

† The distinction of rain, into "rain of the *decomposition*," and "rain of the *recomposition*" of air, by Mr Van Mons, has induced me to inquire, What is the electric state of rain with a rising, and what with a falling barometer?

IX.

METEOROLOGICAL JOURNAL.

		PRESSURE.			TEMPERATURE.					
	Wind	Max.	Min.	Med.	Max.	Min	Med.	Evap.	Rain	
JULY										
12	N W	30·03	29·91	24·97	76	59	67·5	—		☽
13	W.	29·91	29·79	29·85	75	60	67·5	·24	.42	
14	S W	29·83	29·76	29·795	65	59	62	—	—	
15	S	29·83	29·80	29·815	71	59	65	—	—	
16	S W	29·85	29·83	29·84	70	54	63	·43	—	
17	S	29·85	29·75	29·80	72	54	63	—	·12	
18	S W	——	——	——	71	60	65·5	—	·57	
19	S E	29·94	29·75	29·845	73	54	63·5	—	—	
20	W	29·94	29·90	29·92	64	53	58·5	·47	·79	●
21	Var.	29·88	29·82	29·85	60	53	56·5	—	1·61	
22	W	30·01	29·88	29·945	70	50	60	—		
23	N W	30·11	30·01	30·06	69	56	59·5	—		
24	N W	30 15	30·11	30·13	72	54	63	—		
25	N W	30·14	30·12	30·13	73	55	64	·57		
26	S W	30·12	30·09	30·105	72	51	61·5	—		
27	N	30·09	29·91	30·00	74	55	64·5	—		
28	S E	29·91	29·85	29·88	78	54	61	·32		☾
29	N E	30·11	29·85	29 93	75	54	64·5	—		
30	N	30·11	30·08	30·095	67	49	58	—		
31	N E	30·08	——	——	68	52	60	·42		
AUG.										
1	N E	——	29·90	29·99	76	53	64·5	—		
2	S	29·90	29·69	29·795	73	51	62	—		
3	S W	29·67	29·58	29·625	76	51	63·5	·43	·11	
4	N W	29·73	29·60	29·665	68	54	61	—	·27	○
5	S	29·65	29·62	29·635	66	52	59	·29		
6	S	29·59	29·48	29·535	63	50	56·5	—	·26	
7	N W	29·60	29·50	29.55	67	54	60·5	—	·15	
8	S W	29·49	29·35	29·42	62	50	56	·32	·74	
9	N W	29·60	29·48	29·54	64	45	54·5	—	·33	
10	N W	29·86	29·60	29·73	61	44	52·5	·29	—	
		30·15	29·35	29·835	78	44	61	3·78	5·37	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES

NOTES.

July 15. Small rain about 2 p. m. 19. A thunder shower early: fine day. 20, 21. Forty-eight hours rain. 22. Temperature 60°, (the *maximum* of the period) at 8 a. m. 26. Orange-coloured *cirri* at sunset. 27. Thunder clouds: a few drops p. m.: much dew. 28. *Cirrocumulus* cloud, very beautiful, interchanging with *cirrostratus*, succeeded by large *cumuli*. In the evening some appearance of a thunder storm far in the N. W. 29. Evening parallel bars of *cirrostratus*, stretching E. and W.: a blush on the twilight. 30. Windy, cloudy.

Aug. 2. Large elevated *cirri*. 3. *Cirrocumulus*, followed by *cirrostratus*: evening overcast: rain by night. 4. Windy, at S.W. by night. *Cumulostrati*, in various quarters, at sunset. 7. Opaque twilight, with *cumulostratus*. 8. Very wet, a. m.; at noon a thunder shower; at 6 p. m. a heavy squall from N.W. with rain and hail; the *nimbus*, as it receded, presenting a perfect and brilliant bow: windy night. 9. Large *cumuli* rose, and at noon inosculated with the clouds in a superior stratum: a thunder shower ensued before 2 p. m., after which appeared the distinct strata again: about 6 p. m. a second thunder shower, long very dense in the S. E., where the bow was conspicuous above an hour. This day was nearly calm. 10. Rain fell again about noon, upon the union of two *strata* of cloud.

RESULTS.

Prevailing winds, westerly.

Barometer:	max. 30·15;	min. 29·35.	Mean 29·835 In.
Thermom.	— 78°	— 44°	— 61°
Evaporation	3·78 In.		
Rain	5·37 In.		

Character of the period changeable, with much rain.

I have the satisfaction to acquaint my readers, that the Meteorological Tables and Remarks, which will hereafter appear in this Journal, will be extracted (as the present has been,) from the journals of Mr. Luke Howard, whose Treatise on Clouds, inserted in the present number, and long known and valued by the public, will make it unnecessary for me to express, in any direct terms, that sentiment of obligation, which myself, and the other cultivators of science, must entertain for his researches. W. N.

X.

Abstract of a Memoir on the Analysis of Vegetable and Animal Substances: by Messrs. GAY-LUSSAC *and* THENARD*.

Mode of analysing vegetable and animal substances.

WHEN we conceived the design of studying the analysis of animal and vegetable substances, the first idea that occurred to us was, to convert, by means of oxigen, vegetable and animal substances into water, carbonic acid, and nitrogen: and on this we fixed our attention. It was evident, that, if we could effect this conversion so as to collect all the gasses, this analysis would attain very great accuracy and simplicity. Two obstacles appeared in the way of this: first, the burning of the hidrogen and carbon of these substances completely; and, secondly, the effecting of this combustion in close vessels.

Difficulties.

Methods of obviating them.

The first we could hope to surmount only by means of metallic oxides, that easily part with their oxigen, or of the hyperoximuriate of potash. A few trials soon led us to prefer this salt, which succeeded beyond our expectations. It was far from being so easy to surmount the second: for we could not attempt the combustion in a retort filled with mercury; since the retort would have burst, had we burned ever so little in this way. It was necessary therefore to contrive an apparatus, in which we could

Apparatus requisite.

1st, Burn parts of a substance so small, that the vessels should not crack:

2dly, Effect such a number of combustions successively, that the results should be very perceptible: and

3dly, Collect the gasses as they were formed.

Description of an apparatus answering these purposes.

An apparatus of this kind we lay before the class. It is formed of three separate pieces. One is a tube of very thick glass, hermetically sealed at the lower end, and open at the upper; about 2 dec. [7·87 in.] long, and 8 mil. [3·15 lines] in diameter. This has a very small tube, likewise of glass, similar to what would be adapted to a retort to receive gasses, joined laterally to it by means of the blowpipe 5 cent.

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[*] Ann de Chim. Vol. LXXIV, p. 47. Read to the Institute the 15th of January, 1810.

[1·97 in.] from its aperture. The second is a brass collar, in which the open end of the large glass tube is fixed by means of a cement, that will not fuse under 40° [104° F.] The third piece is a cock of a peculiar construction, in which all the merit of the apparatus consists. The key of this cock is solid, and may be turned into any position, without giving passage to the air; but about the middle of its length it has a superficial cavity, capable of holding a substance the size of a small pea. This cavity is so contrived, that when uppermost it answers to a small vertical funnel, which enters into the nozzle, and forms as it were its extremity; and when lowermost it communicates with the body of the cock, which is perforated, and screws into the brass collar before mentioned. Thus on putting small fragments of any thing into the funnel, and turning the key, the cavity is filled with them, and conveys them, on continuing to turn it, into the body of the cock, whence they fall into the brass collar, and so to the bottom of the glass tube.

Its application.

If this matter therefore be a mixture of some vegetable substance with hyperoximuriate of potash in suitable proportion, and if the lower part of the glass tube be sufficiently hot, it will scarcely touch it before it is vividly inflamed; when the vegetable substance will be instantaneously destroyed, and converted into water and carbonic acid, which may be collected over mercury, with the superfluous oxigen gas, by means of the small lateral tube.

Preliminary steps.

To perform this operation readily, it is necessary, that the matter should separate entirely from the cavity, and fall to the bottom of the tube. For this purpose it is to be made into small balls, as will presently be described. It is necessary too to inquire, what quantity of hyperoximuriate will be sufficient for burning the vegetable substance completely; and at least half as much more must be used, that the combustion may be perfect.

Preparation of the substance to be analysed.

Analysis of the hyperoximuriate.

But of all the preliminary steps the most important is the analysis of the hyperoximuriate employed, for all the calculations of the experiments are founded in great measure on this analysis.

All this being well understood, it will be easy to conceive, how

Process described.

how a vegetable substance may be analysed with the hyperoximuriate. Let the substance to be analysed be carefully levigated, and let the hyperoximuriate be levigated separately: weigh the quantity of each, dried at the heat of boiling water, in a very sensible balance; mix them intimately, moisten them, and mould them in cylinders; divide these cylinders into small portions, and round them between the fingers like pills; and lastly expose these to the temperature of boiling water for a sufficient time to render them as dry as the powders were before. If the substance to be analysed be a vegetable acid, it must be combined with lime or barytes, before it is mixed with the hyperoximuriate; the salt thus formed is to be analysed, and account taken of the carbonic acid that remains united with the base after the experiment; in fine, if the substance to be analysed contain any thing foreign to its nature, account must be taken of this also.

Thus we know with precision, that a given weight of the mixture answers to a known weight of hyperoximuriate and the substance to be analysed.

Now, to finish the operation, all that is required is, to bring the bottom of the tube to a cherry red heat; to expel all the air by means of a certain number of balls, which need not be weighed, and which are dropped into it one after another; and then to decompose a quantity accurately weighed, and carefully collect all the gasses in phials filled with mercury, and previously measured.

Proof of its accuracy.

If all the phials be of the same size, they will be filled with gas by equal weights of the mixture; and if the gas be examined, it will be found precisely similar, an evident proof of the extreme accuracy of this mode of analysis.

Caution.

During the whole of the process the tube should be kept at the highest degree of heat it can support without fusion, that the gasses may contain no oxicarburetted hidrogen, or as little as possible. In all cases the analysis should be made over mercury. This is a trial which is indispensable. It is sufficient to mix them with a fourth of their bulk of hidrogen, and to take the electric spark in them. As they include a great excess of oxigen, the hidrogen added, of which account must be taken, burns as well as all the oxicarburetted

Analysis of the gasses.

retted hidrogen they may contain; and thus we acquire a certainty, that they no longer consist of any thing but carbonic acid and oxigen, the separation of which is to be effected by means of potash.

Farther precautions.

But this necessity of raising the temperature so high, obliges us, on the other hand, to take some precautions for preventing the cock from being heated. For this purpose the glass tube is passed through a brick, into which it is luted with clay, which has the advantage, at the same time, of rendering the apparatus firm; and besides, a small hollow cylinder is soldered to the body of the cock, to contain water, or ice, which is still better.

Data.

Thus we have all the necessary data for knowing the proportion of the principles of the vegetable substance. We know how much of it has been burned, for we have its weight to half a milligramme, [about eight thousandths of a grain]; we know how much oxigen was required to convert it into water and carbonic acid, since the quantity is the difference between that contained in the hyperoximuriate and that found in the gasses produced: lastly, we know how much carbonic acid has been formed, and can calculate how much water must have been produced.

Analysis of animal substances.

By following the same method of analysis, we may equally determine the proportions of the constituent principles of all animal substances. But as these substances contain nitrogen; and nitrous acid gas would be formed, if an excess of hyperoximuriate were employed for burning them; only such a quantity must be used, as is sufficient to reduce them completely to carbonic acid gas, oxicarburetted hidrogen, and nitrogen, which are to be analysed in the mercurial eudiometer by the common methods, whence we deduce with precision the proportions of the principles of the animal substance itself.

Small quantities used, but the results accurate.

The mode in which we proceed in the analysis of vegetable and animal substances being exactly known, we may say what is the quantity we decompose, without fear of diminishing the reliance, that may be placed on our results. This quantity extended, at most, to 6 dec. [9·27 grs.] If, however, the least doubt should arise respecting their extreme accuracy, we should remove it by observing, that we filled with gas two and sometimes three phials of the same size in succession; that these gasses were absolutely the same, and always came from the same weight of the substance.

Accuracy of an analysis depends chiefly on the nicety of the apparatus and of the method.

We may add, that the precision of an analysis depends much more on the accuracy of the instruments, and of the methods employed, than in the quantity of the substance on which we operate. The analysis of air is more accurate than any analysis of salts, though it is made on two or three hundred times less matter: because in the former, where we judge of weights by very considerable bulks, the errours to which we are liable are perhaps ten or twelve hundred times less sensible than in the second, where we have not this resource. Now, as we convert into gas the substances we analyse, we bring our analyses not merely to the certainty of ordinary mineral analyses, but to that of mineral analyses of the greatest accuracy; particularly as we collect at least a quart of gas, and in our method of proceeding itself find the proof of an extreme accuracy, and of the most trifling errours.

Vegetable substances already analysed.

By this method, and with all the precautions we have mentioned, we have already analysed sixteen vegetable substances; namely, the oxalic, tartarous, mucous, citric, and acetic acids; yellow resin, copal, wax, and olive oil; sugar, gum, starch, sugar of milk, beech wood, oak, and the crystallizable principle of manna. The results we have obtained seem to us highly interesting, for they have led us to three remarkable laws, to which the composition of vegetables is subjected, and which may be expressed as follows.

Laws of vegetable composition.

1. A vegetable substance is always acid, whenever its oxigen is in greater proportion to its hidrogen than would form water.

2. A vegetable substance is always resinous, or oily, or alcoholic, &c., whenever its oxigen is in smaller proportion to its hidrogen than would form water.

3. Lastly, a vegetable substance is neither acid, nor resinous, but analogous to sugar, gum, starch, sugar of milk, woody fibre, or the crystallizable principle of manna, whenever its oxigen is in the same proportion to its hidrogen as would form water.

Thus,

Vegetable acids;

Thus, if we were to suppose, for a moment, that the hidrogen and oxigen were in the state of water in vegetable substances, which we are far from considering as true, vegetable acids would be formed of carbon, water, and oxigen, in different proportions:

resins, &c.

Resins, fixed and volatile oils, alcohol, and ether, would be formed of carbon, water, and hidrogen, also in different proportions: and

sugar, &c.

Lastly, sugar, gum, starch, sugar of milk, woody fibre, and the crystallizable principle of manna, would be formed of carbon and water alone, and would differ only by the greater or less quantity they contained.

This we may show by quoting various analyses of acid and resinous substances, and of substances that are neither acid nor resinous.

Constituent principles of oxalic acid,

A hundred parts of oxalic acid contain.

Carbon	26·566	or	Carbon	26·566
Oxigen	70·689		Oxigen and hidrogen in the proportions that form water	22·872
Hidrogen	2·745		Oxigen in excess	50·562
	100·			100·

and acetic acid.

A hundred parts of acetic acid contain

Carbon	50·224	or	Carbon	50·224
Oxigen	44.147		Oxigen and hidrogen in the proportions that form water	46·911
Hidrogen	5·629		Oxigen in excess	2·865
	100			100

These the two extremes.

Oxalic acid therefore contains more than half its weight of oxigen in excess with respect to its hidrogen; while in acetic acid this excess is not quite three hundredths.

Explanation of certain facts.

These two acids occupy the extremities of the series of vegetable acids: one is the most oxigenised of them, the other the least. This is the reason why so much nitric acid is required to convert sugar, gum, &c., into oxalic acid; why, on the contrary, many vegetable and animal substances so easily produce acetic acid in a number of instances; and why, in particular, wine is changed into vinegar without the formation of any intermediate acid: a phenomenon hitherto unexplained, because vinegar was considered as the most oxigenized of all the acids.

Constituent principles of common resin,

A hundred parts of common resin contain

Carbon	75·944
Hidrogen and oxigen in the proportions that form water	15·156
Hidrogen in excess	8·900
	100

olive oil,

A hundred parts of olive oil contain

Carbon	77·213
Hidrogen and oxigen in the proportions that form water	10·712
Hidrogen in excess	12·075
	100

crystallized sugar,

A hundred parts of crystallized sugar contain

Carbon	40·194		Carbon	40·194
Oxigen	52·101		Hidrogen and oxigen in the proportions that form water	59·806
Hidrogen	7·705	or	Oxigen in excess	0
	100		Hidrogen in excess	0
				100

and beech wood.

A hundred parts of beech wood contain

Carbon	51·192		Carbon	51·192
Oxigen	42·951		Hidrogen and oxigen in the proportions that form water	48·808
Hidrogen	5·857	or	Oxigen in excess	0
	100		Hidrogen in excess	0
				100

Vegetation solidifies water,

These results evince a very important truth, which is, that vegetables, in the act of vegetating, solidify water

entire

or its principles.

entire, or its principles: for, all vegetables being almost wholly composed of woody fibres and mucilage, which contain oxigen and hidrogen in the same proportions as water; it is evident, that, being taken into the vegetable, it combines with charcoal to form them.

Requisites to the fabrication of vegetable matters.

If therefore it were in our power to unite these two substances in all proportions, and to bring their particles to a suitable degree of approximation, we should be able to make with certainty all the vegetable substances, that occupy the mean between acids and resins, as sugar, starch, woody fibre, &c.

Animal substances analysed.

Of animal substances we have hitherto analysed only fibrin, albumen, gelatin, and caseous matter.

General conclusions.

It follows from our analysis, that, in these four substances, and probably in all similar animal substances, hidrogen is in a larger proportion to oxigen than in water: that, the greater the excess of hidrogen they contain, the greater too is the quantity of nitrogen found in them: that these two quantities are almost in the same proportion as in ammonia; and it is probable, that this proportion, to which we come near, really exists; particularly as we always find a little too much hidrogen, and all the errours, to which we are liable, tend to increase the quantity of this principle. The reader may judge of this from the two following analyses.

Constituent principles of fibrin.

A hundred parts of fibrin contain

Carbon	51·675
Hidrogen and oxigen in the proportions that form water	26·607
Hidrogen in excess	5·387
Nitrogen	16·331
	100

and of cheesy matter.

A hundred parts of caseous matter contain

Carbon	57·190
Hidrogen and oxigen in the proportions that form water	18·778
Hidrogen in excess	5·680
Nitrogen	18·352
	100

Admitting

Analogies between the animal and vegetable kingdom.

Admitting this proportion, these substances would correspond, with regard to the rank they hold among animal substances, to the rank occupied by sugar, gum, woody fibre, &c., among vegetable substances: for, as hidrogen and oxigen, the gaseous principles of these, are capable of mutually saturating each other, and forming water; hidrogen, oxigen, and nitrogen, the gaseous principles of those, can also mutually saturate each other, and form water and ammonia: so that carbon, the only fixed principle they all contain, has no property that acts in this saturation. If we allow ourselves to be guided by analogy, in this point of view, we should compare the animal acids with the vegetable acids; and the animal fats, if there be any that contain nitrogen, with vegetable oils and resins; consequently there is not a sufficient quantity of hidrogen in the uric acid to saturate the oxigen and nitrogen this acid contains, or to form water and ammonia by combining with these two substances; and in animal fats the contrary must occur.

The subject to be pursued.

No doubt many more consequences may be drawn from the preceding results: but we reserve for a future paper this inquiry, of the extent and importance of which we are fully aware.

XI.

Chemical Examination of a white, filamentous Substance, found in the Cavities of the Cast Iron that adheres to the Sides of high Furnaces: by Mr. VAUQUELIN*.

Pieces of iron adhering to the sides of the furnace, and containing a white substance,

IN smelting iron ores there are frequently portions of metal, which, beginning to assume the character of iron, and congealing the moment before the iron is drawn off, remain adhering to the sides of the furnace. In these pieces cavities are frequently formed, which are filled with a white filamentous substance, like flexible amianthus.

supposed to be oxide of zinc.

Several metallurgists have spoken of this substance, Grignon in particular has considered it as an oxide of zinc:

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* Ann. de Chim vol. XXVII, p. 192. Extracted from the Ann. des Museum d'Hist. Nat. An. 7.

but he, no doubt, relied on the external appearance, for it does not contain an atom of this metal.

It is not soluble in acids.

To satisfy himself whether it were really oxide of zinc, Mr. Vauquelin boiled some with different acids, but none of them had any action on it: they did not dissolve an atom. This led to a doubt of the truth of the assertion of metallurgists respecting it: and the following experiment convinced him, that they were altogether mistaken.

Treated with potash,

Having heated this substance with thrice its weight of caustic potash in a silver crucible, it was completely fused, and the mass produced was entirely dissolved by water.

and muriatic acid,

The solution supersaturated with very dilute muriatic acid did not become turbid, but was converted into a white transparent jelly by evaporation, which is never the case with zinc.

When this was perfectly desiccated, and the residuum treated with water, a white powder was obtained, which, when washed and dried, did not differ from the original quantity taken a hundredth and half.

It was found to be silex.

This powder exhibited all the characters of the purest silex. No other earth existed in the liquor from which it had been separated, and not even any sensible quantity of oxide of iron.

How is it separated?

The difficulty consisted not in finding the nature of this substance, but how it was formed in the cavities of the iron. How indeed are we to conceive, that the silex, which is always mixed with alumine and lime both in the ores of iron, and in the fluxes employed, should have separated from these earths in a state of such perfect purity, that no perceptible quantity of foreign matter can be discovered with it?

Apparently by sublimation.

The filamentous, and as it were crystallized state of this silex announces, that it was converted into vapour by the violence of the fire, and afterward gently condensed in the parts of the furnace that were less hot.

This would prove, not only that silex is volatile at a sufficient temperature, but that it is more so than alumine or lime; unless we suppose these two earths to have been raised to a greater height, which is not probable.

XII.

XII.

An Account of the Burrknot Apple. In a Letter to HENRY GRIMSTON, *Esq. F. H. S. By the Rev.* JOHN SIMPSON*.

MY DEAR SIR,

Burrknot appletree. YOUR letter met me on my return home after a month's ramble among the mountains and lakes in Cumberland, and I now send you a short description of the apple tree called here the burrknot. At a proper season I will forward to you a few knots, or knobs of it, for trial, which, put into the ground, will make a long shoot, the following spring; or, if you wish it, I will send you a few knobbed branches with blossom buds upon them, which will bear a little the same year, but you must observe the smaller knobbed branches with blossom buds will not make such fine or handsome trees as the others.

Its good qualities. The burrknot apple tree† is uncommonly productive. My trees never miss bearing, not being so liable to blight in inclement seasons, as other varieties. The fruit is large, its tints resembling the ribston pippin, and about its size. For culinary uses, it is not inferior to the choicest codlin, and a much better keeper. The tree is not liable to canker, owing, I am persuaded, to its not putting out a tap-root, but spreading its numerous fibres from the knob horizontally, and following the richness of the soil.

It bears in a year's growth. Our late worthy and valuable friend, Sir Christopher Sykes, observing my trees of one year's growth with fruit upon them, was astonished, and the following year had the pleasure of exhibiting some of the knobbed branches, which I gave him, adorned with fruit in his own garden to his friends, of which you have probably been an eye witness, having visited so frequently in his time at Sledmere. If you wish for any other information that I can give respecting this apple-tree, I shall be happy to send it, and remain,

Dear sir,

Rooss, near Patrington, Yours very truly,

July 25th, 1808. JOHN SIMPSON.

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* Trans. of the Horticultural Soc. Vol. I, p. 120.

† Specimens of the fruit, and branches of this apple tree from Rooss, which is also plentiful in Lord Hawkesbury's garden at Combe, were exhibited at the meeting of the Society, held Dec. 6th.

XIII.

XIII.

A short Account of a new Apple, called the Spring Grove Codling. By the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. P.R.S. &c.*[*]

Spring Grove codling.

AT the request of Mr. T. A. Knight, I beg leave to lay before this Society the opinion formed by my friends and myself last autumn, on the merits of an apple produced by one of his judicious mixtures, which he has done me the honour to call the Spring Grove codling.

In the beginning of September, I received a small box of these apples, which were fully ripe; when baked they had all the quickness and flavour of the best winter apples, and a considerable tinge of red.

Its season of ripening.

All who had tasted the pie agreed, they had not met with any autumn apple which for baking could be compared to this new one. Mr. Knight informs me, that it is ready for use in the month of July, at a season when London geese are probably better than at any other; but when the old English accompanyment of apple sauce was not, till Mr. Knight furnished us with this apple, possible to be obtained; in this view it becomes an addition of importance to the old English kitchen, the cookery of which true Englishmen still prefer to French ragouts, or to Spanish olios.

Of the burr apple kind

It proves of the burr apple kind, and may be accordingly propagated by cuttings without difficulty, which will bear the next year, as well as by grafting. Mr. Hooker, who colours the Pomona Herefordiensis, has made a very excelcellent representation of this fruit, of which a copy accompanies this communication: as a record in the archives of the Society it may hereafter become a useful, as well as a valuable deposit. The tree grows freely, and bears abundantly.

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[*] Trans. of the Horticultural Society, vol. I, p. 197.

SCIENTIFIC NEWS.

Properties of radiant heat.

DR. Delaroche informs us, that he has made some curious experiments on radiant heat, which he intended to lay before the Institute. He ascertained, that the heat emitted by radiation is not proportional to the excess of the temperature of the radiating body above the circumambient medium, but that it increases in a much more rapid ratio. Thus, taking the quantity of heat emitted with an excess of 87° to be 1°, the quantity emitted with an excess of 900° will be at least 70°. He also found, that the quantity of unluminous calorific rays that traverse glass is much greater in proportion to the total quantity of rays emitted, when the body that emits them is very hot, than when it is less so; and that the nature of the unluminous calorific rays is not identical, but varies according to the temperature of the source that emits them.

Prize subject.

At the request of Mr. Berthollet, the French Institute has proposed for the subject of a prize a determination of the specific heat of gasses.

St. Thomas's and Guy's Hospitals.

Medical and chirurgical lectures.

The Winter Courses of Lectures at these adjoining Hospitals will commence the first week of October; viz.

At St. Thomas's.

Anatomy, and the Operations of Surgery, by Mr. Cline, and Mr. Astley Cooper.

Principles and Practice of Surgery, by Mr. Astley Cooper.

At Guy's.

Practice of Medicine, by Dr. Babington and Dr. Curry.

Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.

Experimental Philosophy, by Mr. Allen.

Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.

Midwifery, and Diseases of Women and Children, by Dr. Haighton.

Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.

Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instructions.

London Hospital.

Dr. Buxton's Autumnal Course of Lectures on the Practice of Medicine will be commenced on Wednesday, the 2d of October.

Anatomical Theatre, Bristol.

Mr. J. Shute will commence his Winter Course of Lectures on Anatomy, Physiology, and the Principles of and Operations in Surgery, on Tuesday, October 1, at eight o'clock in the morning.

Analysis of the mineral water of Bagnère.

Mr. Vergne has lately analysed the mineral waters of St. Felix de Bagnère, near Condat, in the department of the Lot, and the following were the results. Four pounds ten ounces of the water, evaporated to dryness, left a residuum of 113 grains. From this he obtained

Muriat of magnesia	6 grs.
Sulphat of magnesia	41
Sulphat of lime	36
Carbonat of lime	20
Carbonat of iron	1·5
Fatty matter	1

there being a loss of 7·5 grs. The fatty matter had neither taste nor smell; thrown on burning coals, it changed colour, shrunk up like an animal substance, and emitted a very fetid smell of carburetted hidrogen. The heat of the water, taken from the spring at noon on the 21st of June, 1809, was 66·4° F.; and its gaseous products were a moderate quantity of carbonic acid, and still less sulphuretted hidrogen.

Water of Ussat.

The water of the baths of Ussat, near Tarascon, about ten miles from Ax, have been examined by Mr. Figuier, professor of chemistry at Montpellier. He found its heat, taken at several times and at different hours, from 27° R. to

30·5°

30·5° [92·7° to 100·6° F.]. It contained about a sixth of a cubic inch of carbonic acid gas in a pound of water. 12230 grammes yielded, by evaporation, 11 grammes of dry residuum, from which were obtained

Muriate of magnesia	0·42 grammes
Sulphate of magnesia	3·38
Carbonate of magnesia	0·12
Carbonate of lime	3·28
Sulphate of lime	3·75
	10 95

The new spring contained rather less both of carbonic acid and of solid residuum, but the difference was trifling.

The mud collected at the bottom of the baths consisted of

Alumine	40 parts
Carbonate of lime	20
Sulphate of lime	10
Oxided or carbonated iron	2
Silex	28
	100

Water of Niederbrunn.

We have also an analysis of the mineral water of Niederbrunn, in the department of the Lower Rhine, by Professors Gerboin and Hecht, of Strasburg. About half a kilogramme, or one pound *, of this water contained

Muriate of soda	1·8 gramme	= 27·8 gra
Sulphat of lime	0·1	1·54
Carbonate of lime, dissolved in Carbonic acid	0·45	6·95
Carbon. of magnesia, the same	0·21	3·24
Carbon. of iron, the same	0·07	1·08
Muriate of magnesia	0·26	4·02
Muriate of lime	0·345	5·33

Augsburg beer.

In Augsburg and its vicinity, which are celebrated for good beer, it is customary to put into each cask a small bag of the root of the geum urbanum, avens, or herb bennet.

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* Probably the Strasburg pound = 7277 grs. Eng. C.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1811.

ARTICLE I.

On the Destruction of an Enemy's Fleet at Sea by Artillery: by W. MOORE, *Esq., of the Royal Military Academy, Woolwich.*

LEMMA I.

If two Spheres of different Diameters and Different specific Gravities impinge perpendicularly on two uniformly resisting fixed Obstacles and penetrate into them; the Forces which retard the Progress of the Spheres will be as the absolute resisting Forces or Strengths of the Fibres of the Substances directly, and the Diameters and specific Gravities of the Spheres inversely.

Law of resistance to a cannon ball.

Proof.

LET R and r denote the absolute resisting forces of the two substances; F and f the retardative forces; D, d, the diameters of the spheres; Q, q, their quantities of matter; and N and n their respective specific gravities. Then the whole resistances to the spheres, being proportional to the quantities of motion destroyed in a given time, will be as the absolute resisting forces of the two substances and quantities of resisting surfaces jointly; or, as the resisting forces of the substances and squares of the diameters of the

the impinging spheres; because the surfaces of spheres are as the squares of their diameters; that is $\frac{M}{m} = \frac{R}{r} \times \frac{D^2}{d^2}$.

But in general $\frac{M}{m} = \frac{F}{f} \times \frac{Q}{q}$: Therefore equating these two values of the whole resisting forces we have $\frac{F}{f} \times \frac{Q}{q} = \frac{R}{r} \times \frac{D^2}{d^2}$ and $\frac{F}{f} = \frac{R}{r} \times \frac{D^2}{d^2} \times \frac{q}{Q}$; and since the quantities of matter in spheres are in the conjoint ratio of their magnitudes and densities, or of the cubes of their diameters and densities; it is $\frac{F}{f} = \frac{R}{r} \times \frac{D^2}{d^2} \times \frac{d^3}{D^3} \times \frac{n}{N} = \frac{R}{r} \times \frac{d}{D} \times \frac{n}{N}$: that is the forces retarding spheres penetrating uniformly resisting substances are as the absolute strengths of the fibres of the substances directly, and the diameter and specific gravities of the spheres inversely.

Q. E. D.

Lemma II.

Law of the depth to which a ball will penetrate.

The whole Spaces or Depths to which Spheres impinging on differently resisting substances penetrate, are as the Squares of the initial Velocities, the Diameters and specific Gravities of the Spheres directly, and the absolute Strengths of the resisting Substances inversely: or $\frac{S}{s} = \frac{V^2}{v^2} \times \frac{D}{d} \times \frac{N}{n} \times \frac{r}{R}$.

Proof.

For by mechanics $\frac{S}{s} = \frac{V^2}{v^2} \times \frac{f}{F}$; and by the preceding Lemma $\frac{f}{F} = \frac{r}{R} \times \frac{D}{d} \times \frac{N}{n}$; therefore by substitution $\frac{S}{s} = \frac{V^2}{v^2} \times \frac{D}{d} \times \frac{N}{n} \times \frac{r}{R}$.

Q. E. D.

These being premised, I now proceed to resolve the following important

PROBLEM.

To find a general Formula, which shall express the Charge of Powder for any given Piece of Artillery to produce the greatest Destruction possible to an Enemy's Ship at Sea; it being supposed of Oak Substance of given Thickness, and at a Distance not affecting the initial Velocity of the Shot.

To find the charge of powder, that shall do most execution.

By the last of the foregoing lemmata we have generally $V = \left(\frac{S d n R v^2}{s D N r}\right)^{\frac{1}{2}}$. Also the charges of powder vary as the squares of the velocity and weight of the ball jointly. Hence, since it has been determined from experiment, that a charge of half a pound impelled a shot weighing 1lb. with a velocity of 1600 feet per second; we shall, considering V the velocity of any ball impinging on the side of the vessel, have for the expression of the charge impelling it through the space $S = \frac{S R d n v^2 w}{2 D N r s 1600^2}$.

Now to apply this in the present instance it is first necessary, that a case be known concerning the penetration of a given shot into oak substance. Such a case is presented at p. 273 of Dr. Hutton's Robins's New Principles of Gunnery. It is there asserted, that an 18 pounder cast iron ball penetrated a block of well seasoned oak (such as ships of war are generally built with) to the depth of 3½ inches when fired with a velocity of 400 feet per second. Making therefore this the standard of comparison for all cases where the object is of oak substance, we shall have for the charge generally,

$$\frac{400^2 \times \cdot 42}{2 \times 1600^2 \times \frac{7}{24}} \times \frac{S R n w}{D N r}$$

or, because the balls are of the same specific gravity, and the substance the same, or $R = r$, and $N = n$; it will be

$$\frac{400^2 \times \cdot 42}{2 \times 1600^2 \times \frac{7}{24}} \times \frac{S w}{D} = \cdot 045 \times \frac{S w}{D}$$

that is, the charge varies as the space to be penetrated and weight of the ball directly, and diameter of the ball inversely.

But the charge by the problem being to produce the greatest effect possible in the destruction of the vessel; S in the above formula must always be put equal to the given thickness of the side; since it is well ascertained, that, for a shot to produce the most damage to any splintering object, such as oak; it must lose all its motion just as it ceases to be resisted by the object, which happens when the ball has forced its first hemisphere out of the farther surface of it. And the quantity of motion destroyed during the penetration of the first hemisphere of the ball into, and the exit of the same out of the object is precisely equal to what would be destroyed during the penetration of the ball through one of its radii, if the quantity of resisting surface was equal to half its entire superficies. Hence the charge in question will be

$$\cdot045 \times \frac{Sw}{D}$$

S being the thickness of the side of the ship; w the weight of the ball; and D its diameter.

EXAMPLE.

Example

An enemy's ship is in sight: required the charge for the 42 pounder guns to destroy her as quickly and completely as possible, when the ships have approached near to each other. The side of the enemy's vessel, a 74, being 1¾ foot thick of oak timber.

The diameter of a 42 pounder of cast iron being $= \cdot557$ feet; we get

$$\cdot045 \times \frac{Sw}{D} = \cdot045 \times \frac{\frac{7}{4} \times 42}{\cdot557} = 5\cdot93806 \text{ lbs.}$$

or, 5 lbs. 15 ozs. for the weight of the charge sought.

TABLE

TABLE

Containing the various charges for the 12, 18, 24, 32, 36, and 42 pounder guns, for producing the greatest effect in all cases of close action: the substance or object being of oak materials from the thickness of 1 foot to that of 5 feet regularly ascending by 1 in the inches.

Tables of charges for different guns for different thicknesses of a ship's side.

Nature of ordnance.	Thickness of the side of the vessel			
	1 ft.	1 ft. 1 in.	1 ft. 2 in.	1 ft. 3 in.
pounder.	lbs.	lbs.	lbs.	lbs.
12	1·439242	1·559178	1·679116	1·799052
18	1·928571	2·089285	2·249999	2·410714
24	2·336650	2·531371	2·726091	2·920813
32	2·830470	3·066343	3·302215	3·538088
36	3·061630	3·316766	3·571901	3·827038
42	3·393180	3·675949	3·958710	4·241475

	16 inches	17 inches	18 inches	19 inches
	lbs.	lbs.	lbs.	lbs.
12	1·918987	2·038926	2·158863	2·278800
18	2·571428	2·732142	2·892856	3·053571
24	3·115533	3·310254	3·504975	3·699696
32	3·773960	4·009833	4·245705	4·481578
36	4·082173	4·337310	4·592445	4·847581
42	4·524240	4·806905	5·089770	5·372535

	20 inches	21 inches	22 inches	23 inches
	lbs.	lbs.	lbs.	lbs.
12	2·398737	2·518674	2·638612	2·758547
18	3·214285	3·374999	3·535714	3·696428
24	3·894417	4·089137	4·283859	4·478580
32	4·717350	4·953323	5·189195	5·425068
36	5·102717	5·357853	5·612988	5·868124
42	5·655300	5·938065	6·220830	6·670262

Nature of ordn.	Thickness of the side of the vessel.			
	24 inches	25 inches	26 inches	27 inches
Pounder	lbs.	lbs.	lbs.	lbs.
12	2·878484	2·998420	3·118358	3·238292
18	3·857142	4·017856	4·178570	4·339284
24	4·673300	4·868021	5.062741	5·257463
32	5·660940	5·896813	6·132685	6·368559
36	6·123260	6·378306	6·633531	6·888608
42	6·786360	7·069125	7·351890	7·634655

	2 ft. 4 in.	2 ft. 5 in.	2 ft. 6 in.	2 ft. 7 in.
	lbs.	lbs.	lbs.	lbs.
12	3·358228	3·478164	3·598100	3·718036
18	4·521340	4·682054	4·842768	5·003482
24	5·452184	5·646905	5·841626	6·036347
32	6·504432	6·840305	7·076178	7·312051
36	7·143804	7·398940	7·654076	7·909212
42	7·917420	8·200185	8·482950	8·765715

	2 ft. 8 in.	2 ft. 9 in.	2 ft. 10 in.	2 ft. 11 in.
	lbs.	lbs.	lbs.	lbs.
12	3·837072	3·957908	4·077844	4·197780
18	5·164196	5·324910	5·485624	5·646338
24	6·231068	6·425789	6·620510	6·815231
32	7·547924	7·783797	8·019670	8·255543
36	8·164348	8·419484	8·674620	8.929756
42	9·048490	9·331245	9·614010	9·896775

	3 ft. 0 in.	3 ft. 1 in.	3 ft. 2 in.	3 ft. 3 in.
	lbs.	lbs.	lbs.	lbs.
12	4·317716	4·437652	4·557588	4·677524
18	5·807052	5·967766	6·128480	6·289194
24	7·009952	7·204673	7·399394	7·594115
32	8·491416	8·727289	8·963162	9·199035
36	9·184892	9·440028	9·695164	9·950300
42	10·179540	10·462305	10·745070	11·027335

Nature of ordn.	Thickness of the side of the vessel.			
	3ft. 4in.	3ft. 5in.	3ft. 6in.	3ft. 7in.
pounder	lbs.	lbs.	lbs.	lbs.
12	4·797460	4·917396	5·037332	5·157268
18	6·449908	6·610622	6·771336	6·932050
24	7·788836	7·983557	8·178278	8·372999
32	9·434908	9·670781	9·906654	10·142527
36	10·205436	10.460572	10·715708	10·970844
42	11·310600	11·593365	11·876130	12·158895

	3ft. 8in.	3ft. 9in.	3ft. 10in.	3ft. 11in.
	lbs.	lbs.	lbs.	lbs.
12	5·277204	5·397140	5·517076	5·637012
18	7·092764	7·253478	7·414192	7·574906
24	8·567720	8·762441	8·957162	9·151883
32	10·378400	10·614273	10·850146	11·086019
36	11·225980	11·481116	11·736252	11·991388
42	12·441660	12·724425	13·007190	13·289955

	4ft. 0in.	4ft. 1in.	4ft. 2in.	4ft. 3in.
	lbs.	lbs.	lbs.	lbs.
12	5·756948	5·876884	5·996820	6·116756
18	7·735620	7·896334	8·057048	8·217762
24	9·346604	9·541325	9 736046	9·930767
32	11·321892	11·557765	11·793638	12·029511
36	12·246524	12·501660	12·756796	13·011932
42	13·572720	13·855485	14·138250	14·421015

	4ft. 4in.	4ft. 5in.	4ft. 6in,
	lbs.	lbs.	lbs.
12	6·236692	6·356628	6·476564
18	8·378476	8·359190	8·699904
24	10·125488	10·320209	10·514930
32	12·265384	12·501257	12·737130
36	13·267068	13·522204	13·777340
42	14·703780	14·986545	15·269310

Nature of ordnance.	Thickness of the side of the vessel.		
	4 ft. 7 in.	4 ft. 8 in.	4 ft. 9 in.
Pounder.	lbs.	lbs.	lbs.
12	6·596500	6·716436	6 830372
18	8·860618	9·021332	9·182046
24	10·709651	10·904572	11·099093
32	12·973063	13 . 08876	13·444749
36	14·032476	14·287612	14·542748
42	15·552070	15 834840	16·117605

	4 ft. 10 in.	4 ft. 11 in.	5 ft. 0 in.
	lbs.	lbs.	lbs.
12	6·956308	7·076244	7·196180
18	9·342760	9·503474	9·664188
24	11·293814	11·488535	11·683256
32	10·680622	13·910495	14·152368
36	14·797884	15·053020	15·308156
42	16·400370	16 683135	16·965900

Explanation of the table.

In this table the first column contains the nature of the ordnance, and the numbers in the other columns are their respective charges of gunpowder in pounds, when the thickness of the object to be destroyed is as specified at the top of the columns. If the thickness be given in inches and parts of inches take such parts of the difference between the charge for the given number of inches and the next greater, and add them to the charge first found for the given number of inches for the charge required.

The value of the decimal part of each will be had by multiplying it by 16, the number of ounces in a pound, and pointing off in the product from the right hand towards the left as many places for decimals as are contained in the given decimal, and retaining the number on the left of the point for the ounces, increasing it by ¼, ½, ¾, or 1, when the first figure of the decimal is 2, 5, 7, or 8, respectively. This hint is merely given for those practitioners into whose hands this table may fall, who are not very conversant in decimal arithmetic.

SCHOLIUM.

SCHOLIUM.

The problem applicable to military as well as naval operations.

Advantage of a proper charge in a seafight,

and disadvantage of too much powder.

This problem is not only of the utmost importance, and practically useful in naval engagements, but in several instances also of military operations; as the bursting open gates of besieged cities with promptitude and effect, and breaking up all fortifications composed of wooden materials, especially those of a splintering nature, to which the foregoing charges apply most correctly. In the case of a naval action, where the object to be penetrated is of oak substance, the ball, by having a small motion when it quits the ship's side, tears and splinters it excessively, breaking away large pieces before it, which are not so easily supplied in the reparation, whereas, on the other hand, if the shot had any considerable velocity when it quitted the side, the effect it produced would be merely a hole, which would be stopped instantly by the mechanic employed for that purpose, and indeed in a great measure by the springiness of the wood itself; for I have seen in his Majesty's dock-yard at Woolwich, captured men of war having a number of shot holes in them almost wholly closed by the wood's own efforts; and that required nothing more than a small wooden peg or a piece of cork to stop them up perfectly. All the mischief therefore the balls can do under such circumstances of extreme celerity is, merely killing those men who may chance to stand in the way of their motion.

Cases of thicker substances, or stone or brick walls.

If any object to be destroyed be so thick, that it cannot be completely pierced by any common engine; or if it be of a very brittle nature, such as stone or brick; then that charge is to be used, which will give the greatest velocity to the shot, to produce the greatest effect. But in many cases of bombardment this charge is by no means to be preferred; for though the effect produced each individual time be greater, yet in any considerable time the whole effect would be less than that from a smaller charge oftener fired, on account of the extreme heat it would give to the engine after a few discharges; and in consequence of which greater time would be required for cooling the gun, and preparing it for farther service.

EXAMPLE II.

Case of bursting open a gate with a 24 pounder.

Required the charge for a 24 pounder shot to burst open the gates of a city with the greatest ease possible, the substance of them being elm 1 foot thick.

Here the object to be penetrated being elm, the small letters in the general formula for the charge

$$\frac{S\,d\,v^2\,w}{2\,D\,s \times 1600^2}$$

must be made to express the several numbers of some experiment made in the penetration of this substance. Now by a mean of many very accurate experiments made by Dr. Hutton at Woolwich, in the years 1783, 1784, and 1785, he found, that a cast iron ball of two inches diameter impinging perpendicularly on the face of a block of elm-wood, with a velocity of 1500 feet per second, penetrated [illegible] inches deep into its substance; hence we shall have $d = \frac{1}{6}$ ft. $v = 1500$, and $s = \frac{13}{12}$ ft.; also by the question $S = 1$ ft. $D = {\cdot}46$, and $w = 24$lbs. Therefore

$$\frac{S\,d\,v^2\,w}{2\,D\,s \times 1600^2} = \frac{1 \times \frac{1}{6} \times 1500^2 \times 24}{2 \times {\cdot}46 \times \frac{13}{12} \times 1600^2} = \frac{45 \times 9}{104 \times 1{\cdot}11}$$

$= 3{\cdot}50831$ lbs. or 3 lbs. $8\frac{1}{8}$ ozs. for the weight of the charge required in this case.

Retaining the experiment of Dr. Hutton as a standard for all cases where the object to be penetrated is of elm, we shall get by reduction

$$\frac{S\,d\,v^2\,w}{2\,D\,s \times 1600^2} = {\cdot}0676 \times \frac{S\,w}{D}$$

the charge for any piece of artillery the diameter of the shot of which is D, and weight w; S being the thickness of the object as before.

A gate may be burst by the recoil of a gun.

It is not unworthy of remark, that the gate of a besieged place, or any like things, might be effectually broken open by the gun itself charged only with powder, by placing it close to the gates with its muzzle from them; the momentum of recoil being generally sufficient to force such objects completely.

Of great importance in close fighting.

From the circumstance, that no English admiral or commander ever commences firing till his ships are about to be grappled with those of the enemy, or until they have approached them so nearly as to affect in no sensible degree the first force of the shot; the above paper has, it is presumed, as much claim to utility as any that has ever yet been offered to the navy in the science of gunnery: and even if the vessels be not so closely in action, but are fighting at the distance of about 30 or 40 feet from each other, no danger would result from the above charges, provided that the shot impinged perpendicularly on the side of the vessel; on account of the splitting of the timber in some degree, which would make ample compensation for the defect of velocity occasioned by the resistance of the medium.

Distant Eng.

It is impossible to deduce charges, that shall produce with certainty the effect above stated when fired at any considerable distance from the ship. The uncertainty of the impact being perpendicular from the unsteadiness of the vessels renders the thing at once nugatory, without any consideration of the *real* resistance of the medium to the ball, and the deflection of the latter from a right lined direction. If the obliquity of the impact be given, or can be determined, then, the problem being otherwise rightly solved, a charge can be found, which shall answer the same purpose as those above given; but, if this be impossible (which it most decidedly is), then will the problem be at best but speculative upon certain hypotheses.

I shall however give an investigation of the problem on the principles of resistance generally allowed, and then conclude the subject by a few observations.

PROBLEM II.

To determine the same as in the last Problem, when the Engine is at any considerable Distance from the Object, and the Resistance of the Air taken into the account.

To find the charge, that shall do most execution at a distance.

Here, as in the former proposition, the Velocity $V = \left(\frac{S\,dr^2}{Ds}\right)^{\frac{1}{2}}$ is to be esteemed the velocity of impact. Now on the principle of resistance just adverted to, which considers the fluid as infinitely compressed, and the particles thereof perfectly nonelastic and affording no resistance to the body but what arises from their inertia. If a denotes the first or initial velocity; x the distance of the gun from the object; $c = 2{\cdot}71828$ the number the hyperbolic log. of which is unity; and $b = \frac{3n}{8ND}$ where N and n represent the respective specific gravities of the ball and medium, we shall have

$$a = V c^{bx}$$

(See Dr. Hutton's elegant Exercises on Forces, Prob. 31, and most works on fluxions and mechanics). Hence by the law of variation of the charges, and proper substitution, the true expression for the charge in question will be

$$\frac{S d v^2 w c^{\frac{3nx}{4ND}}}{2 D s\, 1600^2}$$

for a perpendicular impact, and

$$\frac{S d v^2 w c^{\frac{3nx}{4ND}}}{2 D s f\, 1600^2}$$

for an oblique one; f being the sine of the angle of incidence; the space to be described in this case being the hypothenuse of a right angled triangle; when the effect is the same.

EXAMPLE.

Charge for a 42 pounder at 100 yards distance.

Resuming the first of the foregoing examples; what must be the charge of gunpowder to cause the shot to produce the same effect in the vessel when fired at the distance of 300 feet from it?

Substituting for the several letters in the general expression for the charge

$$\frac{S d v^2 w c^{\frac{3nx}{4ND}}}{2 D s\, 1600^2}$$

their proper numerical values, namely

S =

$S = 1\frac{3}{4}$ ft.
$s = \frac{13}{14}$ ft.
$d = \frac{1}{8}$ ft.
$D = {\cdot}557$. ft.
$v = 1500$ ft.
$x = 300$ ft.
$w = 42$ lbs.
$N = 7\frac{1}{3}$
$n = {\cdot}0012$

we get $$\frac{S d v^2 w c^{\frac{s n x}{4 N D}}}{2 D s\, 1600^2} =$$ 9·530695 lbs. or 9 lbs. 8½ ozs. nearly for the weight of charge sought; being 3 lbs. 9½ ozs. more in this case than when the vessels are in close action.

Hence not only is the destruction of the vessel more certain when the firing commences just as the ships touch each other but a great saving of powder takes place beside, insomuch, that not more than two thirds of the quantity is expended, that would be required at the distance of 300 feet.

From this circumstance then, and the impossibility of solving the problem rightly from the various causes already enumerated, the effects of which are not reducible to any regular laws; we conclude, that the foregoing table of charges for close fighting is the only one, that can be of the smallest service in practice, and that all attempts at others must be rendered completely futile from the nature and constitution of things.

II.

Correction of an Errour in a former Paper on the Motion of Rockets. By W. Moore, *Esq. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

I TAKE the earliest opportunity of correcting the errour so obligingly mentioned by *Zeno* in the last number of your Journal: into which I inadvertently fell in my paper concerning Rockets for July last.

On the motion of rockets.

Conceive Q R and K n (Pl. 8, fig. 2,) erased from the diagram, and Q W drawn perp. to T n produced in the plane T Q W; also, draw W R perp. to T P and join P W which will be perp. to T W. Then calling T P unity, T Q will be $= f$ (the same substitution for the several angles remaining as before); also sine ∠ T Q W being expressed by $\frac{x}{r}$ by Trig. $TW = \frac{fx}{r}$: hence $PW = (TP^2 - TW^2)^{\frac{1}{2}}$

On the motion of rockets.

$= \left(1 - \frac{ſ^2 x^2}{r^2}\right)^{\frac{1}{2}} = \frac{(r^2 - ſ^2 x^2)^{\frac{1}{2}}}{r}$ which in the present case is equal to the sine of the angle P T W or P W R.

Now because of the oblique action of the fluid against the cylinder, (considering the fluid in motion, and the solid at rest) its force on this account will be diminished in the ratio of radius to the cube of the sine of the angle of incidence, or as 1 to $ſ^3$. Therefore considering T P the representative of the force of a particle so diminished $\left(= \frac{n v^2 ſ^3}{4g}\right)$; its efficacy to move the cylinder in the direction P T will be P R and $= \frac{n v^2 ſ^3}{4g} \cdot \sin^2 \angle \text{PWR} = \frac{n r^2 ſ^3}{4g} \cdot \frac{r^2 - ſ^2 x^2}{r^2} = \frac{n v^2 ſ^3}{4 g r^2} \cdot (r^2 - ſ^2 x^2)$. Therefore the fluxion of the force of the fluid on F T will be

$$\frac{n v^2 ſ^3}{4 g r^2} \cdot (r^2 - ſ^2 x^2) \cdot \frac{r \dot{x}}{(r^2 - x^2)^{\frac{1}{2}}} \cdot \frac{(r^2 - ſ^2 x^2)^{\frac{1}{2}}}{r}$$

or

$$\frac{n v^2 ſ^3}{4 g r^2} \cdot \frac{\dot{x}\,(r^2 - ſ^2 x^2)^{\frac{1}{2}}}{(r^2 - x^2)^{\frac{1}{2}}}$$

the fluent of which is

$\frac{n v^2 ſ^3}{4 g r^2} \cdot \left(r^2 x - \frac{3 ſ^2 - 1}{6} x^3 + \frac{3(ſ^2 - 1)^2}{40 r^2} x^5 + \frac{(ſ^2 + 5) \cdot (ſ^2 - 1)^2}{112 r^4} x^7 + \text{\&c.}\right)$ which when $x = r$ is $\frac{n v^2 r ſ^3}{4g} \left(1 - \frac{3 ſ^2 - 1}{6} + \frac{3(ſ^2 - 1)^2}{40} + \frac{(ſ^2 + 5) \cdot (ſ^2 - 1)^2}{112} + \text{\&c.}\right)$. This therefore is the effective force of the fluid on the quadrantal arch F T S. Hence the force on the whole semicylindric surface m D v r B s $= \frac{n v^2 r h ſ^3}{2g} \left(1 - \frac{3 ſ^2 - 1}{6} + \frac{3(ſ^2 - 1)^2}{40} + \frac{(ſ^2 + 5) \cdot (ſ^2 - 1)^2}{112} + \text{\&c.}\right)$ which is also the resistance to the cylinder when this moves in the fluid at rest, as far as relates to that surface. Q. E. I.

Yours, &c.

W. MOORE.

III.

On a Property of reflected Light: by Mr. MALUS*.

Reflected or refracted rays generally similar to direct;

but not always.

WHEN a solar ray is reflected, or refracted, it retains in general its physical properties; and if it be subjected to new trials, it comports itself in the same manner, as if it issued directly from the luminous body. The prism, while it disperses the coloured rays, only changes their respective directions, without altering their nature. There are circumstances however, in which the influence of certain bodies impresses on the rays they reflect, or refract, characters and properties which they carry with them, and by which they are essentially distinguished from direct light.

Double refraction

The property of light I am about to describe is a modification of this kind. It had already been perceived in a particular circumstance of the doubling of images exhibited by calcareous spar: but, the phenomenon resulting from it having been ascribed to the properties of this crystal, no one suspected, that it might be produced, not only by all bodies that afford a double refraction, but by all other diaphanous substances, whether solid or liquid, and even by opake bodies.

A ray of light received on Iceland crystal is divided into two.

Plane of the principal section passes through them.

The rays refracted ordinarily and extraordinarily

If a ray of light be received perpendicularly on the face of a rhomboid of calcareous spar, this ray is divided into two pencils, one continued in the direction of the incidental rays, the other making with it an angle of a few degrees. The plane that passes through these two rays has several peculiar properties, and is called the plane of the principal section. It is always parallel to the axis of the integrant particles of the crystal, and perpendicular to the natural and artificial refractive surface. When the incident ray is inclined to the refractive surface, it is equally divided into two pencils; one refracted according to the ordinary law, and the other according to an extraordinary law, which depend on the angles that the incident ray forms with the refractive surface and the principal section. When the face of emergence is parallel to that of incidence, the two emergent rays are parallel to the incident ray, because each ray undergoes the same kind of refraction at the two opposite faces.

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* Mém. de la Soc. d'Arcueil, vol. II, p. 143.

The two rays received on another crystal are not divided, when their principal sections are parallel.

If now we receive on a second rhomboid, the principal section of which is parallel to that of the first, the two rays that have already passed through this, they will no longer be divided into two pencils, as rays of direct light would. The pencil from the ordinary refraction of the first crystal will be refracted by the second according to the law of the ordinary refraction, as if this crystal had lost the faculty of doubling images. In like manner the pencil from the extraordinary refraction of the first crystal will be refracted by the second according to the law of the extraordinary refraction.

But by altering the position of one of the crystals they are divided,

If, the first crystal remaining fixed, we turn the second so, that the face of incidence shall remain parallel with itself, each of the two rays arising from the refraction of the first crystal begins to divide itself into two pencils; so that one portion of the ray from the ordinary refraction, for example, begins to be refracted extraordinarily, which produces four images. Finally, after a quarter of a revolution, the pencil from the ordinary refraction of the first crystal is entirely refracted extraordinarily by the second; and, vice versa, the pencil, from the extraordinary refraction of the first crystal is wholly refracted according to the ordinary law by the second; which again reduces the number of images to two. This phenomenon is independent of the angles of incidence, since during the movement of the second crystal the refractive faces of the two rhomboids preserve the same inclination toward each other.

and again reduced to two.

Distinction between direct and refracted light.

Thus the character that distinguishes direct light from light that has been subjected to the action of a crystal is, that the one constantly possesses the faculty of being divided into two pencils, while in the other this faculty depends on the angle comprised between the plane of incidence and that of the principal section.

Light affected in the same way by all double refracting substances.

This faculty of altering the character of light, and of impressing on it a new property, which it carries with it, is not peculiar to the Iceland spar: I have found it in all known substances that double images; and, what is remarkable in this phenomenon, it is not necessary for its production to employ two crystals of the same kind. Thus the second crystal, for example, may be carbonate of lead, or sulphate of barytes; the first may be a crystal of a sulphur, and the second of rock crystal. All these substances comport themselves with one another in the same manner as two rhomboids of calcareous spar. In general this propensity of light to be refracted in two pencils, or in one only, depends solely on the respective positions of the axis of the integrant particles of the crystals employed, be their chemical principles what they may, and of the natural or artificial faces, on which the refraction is produced. This result proves, that the modification light receives from these different substances is perfectly identical.

Method of rendering the phenomena more evident.

To render the phenomena I have described more sensible, the flame of a taper may be viewed through two prisms of different substances, possessing the property of double refraction, placed on each other. In general we shall perceive four images of the flame: but, if we turn one of the prisms slowly round the visual ray as an axis, the four images will be reduced to two, as often as the principal sections of the contiguous faces become parallel, or cut each other at right angles. The two images that disappear do not lose themselves in the other two; we perceive them gradually become extinct, while the other acquire increased intensity. When the two principal sections are parallel, one of the images is formed by rays refracted in the ordinary way by the two prisms, and the other by rays refracted extraordinarily. When the two principal sections are perpendicular, one of the images is formed by rays refracted ordinarily by the first crystal, and extraordinarily by the second; and the other by rays refracted extraordinarily by the first crystal, and ordinarily by the second.

Light affected in a similar way by all transparent bodies, and even by opake ones.

Not only all crystals, that double images, are capable of giving light this faculty of being refracted in two pencils, or in one only, according to the position of the refractive crystal; but all transparent bodies, whether solid or liquid, and even opake bodies themselves, can impress on the luminous particles this singular disposition, which seemed to be one of the effects of double refraction.

Partial reflection from transparent bodies.

When a pencil of light traverses a transparent substance, a portion of the rays is reflected by the refractive surface, and another portion by the surface of emergence. The cause of this partial reflection, which has hitherto escaped the researches of natural philosophers, seems, in several circumstances, to have some analogy with the forces that produce the double refraction.

From water.

For example, light reflected by the surface of water under an angle of 52° 45′ with the perpendicular has all the characters of one of the pencils produced by the double refraction of a crystal of calcareous spar, the principal section of which is parallel or perpendicular to the plane, that passes through the incident ray and the reflected ray, which we shall call the plane of reflection.

The reflected ray received on a double refracting crystal.

If this reflected ray be received on any crystal, that has the property of doubling images, and the principal section of which is parallel to the plane of reflection, it will not be divided into two pencils, as a ray of direct light would have been; but it will be refracted entire according to the ordinary law, as if the crystal had lost the faculty of doubling images. If, on the contrary, the principal section of the crystal be perpendicular to the plane of reflection, the reflected ray will be refracted entire according to the extraordinary law. In the intermediate positions it will be divided into two pencils according to the same law, and in the same proportion, as if it had acquired its new character by the influence of the double refraction. The ray reflected by the surface of the liquid therefore, under this circumstance, has all the characters of an ordinary ray formed by a crystal, the principal section of which is perpendicular to the plane of reflection.

This phenomenon analysed.

To analyse this phenomenon completely, I placed a crystal so that its principal section was vertical; and after having divided a luminous ray by means of the double refraction, I received the two pencils proceeding from it on the surface of water, at an angle of 52° 45′. The ordinary ray, in being refracted, gave up to the partial reflection a portion of its particles, as a pencil of direct light would have done; but the extraordinary ray penetrated the liquid entire, and none of its particles escaped refraction. On the contrary, when the principal section of the crystal was perpendicular to the plane of incidence, the extraordinary ray produced alone a partial reflection, and the ordinary ray was refracted entire.

Different bodies produce the effect at different angles.

The angle under which light experiences this modification in being reflected at the surface of different transparent bodies is not the same in all. In general it is greatest in those that refract light most. Above and below this angle a part of the ray is more or less modified, and in a manner analogous to what takes place between two crystals, the principal sections of which cease to be parallel or perpendicular.

Simple exhibition of the phenomenon.

If we would simply observe this phenomenon, without measuring it accurately, we have only to place before a taper the transparent body, or the vessel containing the liquid to be subjected to experiment. We must then observe through a prism of flint glass the image of the flame reflected at the surface of the solid or the liquid, and in general two images will be seen: but on turning the crystal round the visual ray as an axis, one of the images will be seen to grow faint in proportion as the other increases in intensity. Beyond a certain limit, the image that had grown weak begins to renew its intensity at the expense of the second. At the point where the intensity of the light is nearly a minimum, we must move the reflecting body nearer to the taper, or farther from it, till the angle of incidence is such, that one of the two images wholly disappears. This distance being found, if we continue to turn the prism slowly, we shall perceive, that one of the two images becomes extinct alternately at every quarter of a revolution.

The phenomenon takes place in rays reflected in the interior of a substance

The phenomenon I have mentioned in the rays that are reflected under a certain angle at the surface of a transparent body takes place likewise, but under a different angle, with the pencils reflected interiorly by the surface of emergence; and the sine of the first angle is to the sine of the second as the sine of incidence to the sine of refraction. Thus, if we suppose the face of incidence and the face of emergence parallel to each other: and the angle of incidence such, that the ray reflected at the first surface presents the phenomenon I have described; the ray reflected at the second surface will be modified in the same manner. If the incident ray be such, that all its particles escape the partial reflection and pass through the face of entrance, they will equally escape by traversing the face of exit. This new property of light affords the means of measuring with precision the quantity of rays absorbed at the surface of diaphanous bodies, a problem, which the partial reflection rendered almost impossible to be solved.

Light reflected from the surface of a doubly refracting body.

When a body, that produces a double refraction, reflects the light at its first surface, it comports itself like a common transparent substance. The light reflected under a certain angle of incidence acquires the property I have described; and this angle is independant of the position of the principal section, which influences only the double refraction, or the reflections that take place in the interior of the crystal.

Rays reflected interiorly exhibit peculiar phenomena.

In fact, the rays that are reflected interiorly at the second surface exhibit peculiar phenomena, which depend both on the refractive power, and the properties of reflected light that I have already described.

When a pencil of light has been divided into two rays at the first surface of a rhomboid of calcareous spar, these two rays issue out by the second face in two pencils parallel to the incident ray, because each of them experiences at that face the same kind of refraction as at the first face. It is not the same with reflected light. Though the ray refracted ordinarily at the first surface is refracted ordinarily at the second, it is nevertheless reflected at this surface in two pencils, one ordinary, the other extraordinary. In like manner the ray refracted extraordinarily is reflected in two others; so that there are four reflected rays, while there are but two emergent. These four rays, in returning to the first face of the crystal, issue out in four parallel pencils, which make with this face the same angle as the incident ray, but in a contrary direction, and are parallel to the plane of incidence. To connect this kind of reflection with that of double refraction, we must conceive at the two points of emergence of the second face two incident rays, making with this face the same angle as the emergent rays, but in the opposite direction. These two rays, by their refraction through the crystal, will produce four pencils, which will follow precisely the course of the reflected rays. Thus the law of the double refraction being known, that of the double reflection may easily be deduced from it.

Quantity and properties of the light.

I shall now proceed to that kind of phenomenon, which is the subject of this paper; and which relates, not to the law according to which the rays are directed, but to the quantity and properties of the light they contain.

Let us suppose the angle of incidence to be constant, and the crystal placed horizontally. If we turn the rhomboid round the perpendicular, so as to approximate its principal section to the incident rays, we shall perceive a gradual diminution of intensity in the ordinary ray reflected extraordinarily, and of the extraordinary ray reflected ordinarily. In fine, when the plane of the principal section coincides with the incident ray, these two reflected rays disappear entirely, and nothing remains but the ordinary ray reflected ordinarily, and the extraordinary ray reflected extraordinarily. The latter however has much less intensity than the former.

If now, the incident ray continuing to be included in the principal section, we increase or diminish the angle of incidence, till it becomes 56° 30′, the latter reflected ray will disappear altogether; and only that, which has been refracted ordinarily, and reflected ordinarily, will remain. Beyond or within this angle, the extraordinary ray reflected extraordinarily will reappear with an intensity proportional to the remoteness from this angle. The angle of incidence I have mentioned is that, under which a ray reflected at the first surface would have acquired the property of being divided into two pencils, or remaining in one, as takes place at the surface of any other transparent body. The preceding phenomenon may easily be connected with the experiment, in which water was taken for an example: for if we let fall on the surface of the rhomboid, under an angle of 56° 30′, or thereabout, a ray disposed to be refracted only in one extraordinary pencil, this ray will produce no partial reflection at the first surface; which seems to explain, why it produces none at the second.

However, it is not the same, when the plane of incidence makes a sensible angle with the principal section. If the ray just mentioned be made to fall in this plane, under an angle of 56° 30′, or near it, it will comport itself at the first surface as in the preceding case, it will traverse it without any reflection: but at the second surface it will be reflected in two pencils, which will attain their maximum of intensity, when the plane of incidence is perpendicular to the principal section.

It is obvious, that the light reflected at the second face does not comport itself here as in the preceding case, because in the first experiment the incident ray refracted and reflected is still in the same plane, while in the last the repulsive force, that produces the extraordinary refraction, turns the light away from the plane of incidence, so that it ceases to be similarly circumstanced with respect to the forces that act on it.

Light from the partial reflection of opake bodies.

If we examine the light that proceeds from the partial reflection of opake bodies, as black marble, ebony, &c., we shall equally find an angle, at which this light enjoys the properties of that which has traversed a crystal of Iceland spar. Polished metals appear to be the only reflecting substances, that do not seem capable of producing this phenomenon: but, if they do not impress this peculiar disposition on luminous rays, they do not alter it, when they have already acquired it by the influence of another substance.

This property is preserved also by pencils, that traverse substances which refract light singly.

Reflection from metallic mirrors.

In the second part of this paper* I shall describe the circumstances, under which, by means of reflection from metallic mirrors, the mutual disposition of the particles of a ray, either ordinary or extraordinary, may be so changed, that some shall always be refracted ordinarily, while the others are refracted extraordinarily. The examination of these different circumstances will lead us to the law of these phenomena, which depends on a general property of the repulsive forces that act on light.

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* This will appear in our next. C.

IV.

IV.

Experiments on the Transmission of Sound through solid Bodies, and through Air in very long Tubes: by Mr. Biot[*].

It has long been known, that air is not the only medium, in which the phenomenon of sound may be produced and transmitted. All bodies enjoy this property, when they enter into a vibratory motion: and as, even in the most solid substances, the elasticity of the ultimate particles appears to be extremely great, it follows, that sound may be produced and transmitted in all bodies, when they are suitably agitated. This result is confirmed by a great number of daily observations. The miner, when excavating his gallery, hears the strokes of the miner opposed to him: and thus judges of his direction. Stone, wood, metals, and even water, transmit sound: and Franklin assures us, that he has heard under water, at the distance of half a mile, the sound of two stones struck against each other. Several too have observed, that the velocity of sound is much greater in solid bodies, than in the air. Experiments of this kind were made in Denmark on a wire extended horizontally 600 feet. A piece of sonorous metal, suspended from one extremity of this wire, was struck gently; and a person at the other extremity holding it between his teeth, or applying it to some solid part of the organ of hearing, heard two distinct and successive sounds. The first and most rapid was transmitted by the wire: the second through the air: and from their interval, compared with the known velocity of sound in air, it was found, that the sound transmitted by the metal arrived almost instantaneously. These experiments were repeated in England by the Royal Society, and similar results were obtained, but I do not know the precise quantities found. Mr. Hassenfratz too made experiments on the same subject in the quarries at Paris, with Mr. Gay-

Sound produced and transmitted by other bodies beside air:

as the ground,

water,

a wire of 600 feet.

Experiments in stone quarries.

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[*] Mém de la Soc. d'Arcueil, vol. II, p. 405. Read to the Institute November, 1808.

Lussac. A stroke of a hammer against the side of the gallery produced two sounds, which separated at a certain distance, and that transmitted by the stone arrived first. This separation too was observed, when the sound was transmitted through iron bars, or wooden rails of different lengths, and no perceptible interval could be distinguished between giving the stroke and hearing the sound.

None of these show the precise velocity in solids.

All these experiments are well adapted to show the great velocity, with which sound is conveyed through solid bodies, but they were made on lengths not sufficient to afford a measure of this velocity, or even to give a precise idea of it.

Attempt to ascertain it by their vibrations.

An ingenious philosopher, whom we have now the pleasure of having at Paris, Mr. Chladni, author of some very fine experiments on the vibrations of solids, has proposed a method of estimating the transmission of sound through their substance. It consists in causing a rod of any substance, of a given length, to vibrate by friction: when the tone produced by the rod, compared with that of a column of air of the same length, will give the ratio of the velocities of the transmission of sound through air, and through the substance of which the rod is formed. In fact, we readily perceive from the theory, that the velocity of the longitudinal oscillations of a body and that of the sound transmitted through it are proportional to one another: but it is necessary to be certain, that the whole rod vibrates so as to give its fundamental note, without dividing itself into its aliquot parts: for such a separation, heightening proportionally the tone, would give a velocity of sound proportionally above the truth.

16 or 17 times as great as in air.

In this way Mr. Chladni found, that the velocity of sound in certain solid bodies is 16 or 17 times as great as in air. The most elastic substances are iron, and fir with very straight fibres, when it is rubbed longitudinally.

Experiments made on the aqueducts forming at Paris,

The construction of the aqueducts and conduits, which is at present carrying on for the embellishment of the capital, has furnished me with means of making experiments of this kind on a much greater length, than any of those who have gone before me have had at their disposal. It was besides a subject of curiosity, to learn the effects and reach of the human voice in very long cylindrical tubes. Such were the objects of the following experiments. Some of them were made by Mr. Bouvard and me, others by one of us alone. Mr. Malus, colonel of engineers, was likewise present at many of them. In all of them we were assisted by Mr. Martin, maker of nautical watches, a very ingenious and attentive artist, who was particularly appointed to give instantaneously, at determinate seconds, the stroke that was to produce the sound.

which consist of a series of iron pipes.

The sonorous body, on which we operated, was formed by a series of cylindrical tubes of cast iron, of as equal dimensions as possible, and the mean length of which I found to be 2·515 met.* [8 feet 3 in. nearly]. This I found by measuring the whole length of twelve cylinders placed end to end. The tubes are separated by leaden rings covered with tarred fustian: but they are pressed together by strong screws, so that the rings are forcibly compressed, and so close a contact produced, that no water can escape. The mean thickness of each ring is 0·014286 met. [0·562 of an inch], as I found by measuring twelve. The whole series of cylinders forms a curved line, which has two inflexions about the middle of its length: but they were not all joined together at once, and we made our experiments on different successive lengths, as will be seen in my report of them.

1st set of experiments, on a length of 215 yards.

The first were made by Mr. Bouvard and myself on 78 cylinders, forming a length of 196·17 met., to which must be added 1·1 for the 77 rings, giving a total length of 197·27 met. [215·587 yds]. The following were the phenomena we observed.

Apparatus.

In the last cylinder was placed a ring of iron, of the same diameter as the cylinder, and having in its centre a bell without a clapper, and a hammer that could be let fall at will. The hammer, as it struck the bell, struck also the cylinder, with which it formed a communication by means of the iron ring. Two sounds must therefore be heard, one transmitted by the cylinder, the other by the air.

Mode of experimenting.

In fact they were heard very distinctly by applying the ear to the cylinder, and even without this. They appeared sensibly in unison. The first and more rapid was transmitted by the substance of the cylinder, the second by the air. Strokes of a hammer on the last cylinder likewise produced this transmission. We observed attentively with half second chronometers the intervals between the two sounds transmitted. We even employed successively sexagesimal and decimal watches, to vary the numbers observed. Thus we found

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* All the measures employed in this paper are expressed in metres; and the time in seconds of the sexagesimal division.

Differences in velocity of the solid and the air.

In 11 observations	0·527″	Bell.
22	0·555″	Hammer
20	0·544″	Bell.
53 observations. Mean	0·542″	

The interval given by the hammer, and by the bell, appeared to us absolutely the same, without any sensible difference. For this reason we have united them in the same mean. Their tones however were very different. Thus in solid bodies, as in the air, the tone makes no difference in velocity of the sound.

Velocity of transmission through the solid calculated.

The temperature of the air during the experiment was 11° [51·8° F.]. The barometer was about 0·76 [29·9 in.]. In similar circumstances the velocity of sound in the air is 340·84 met. [372·487 yds] according to the experiments of the academy, which give 334·02 met. [365·034 yds] for the velocity under the same pressure, and at the temperature of melting ice. For the distance of 197·27 [215·587 yds] therefore, that at which the experiment was made, the time

of transmission of the sound by the air was ...... 0·579″
The interval observed between the two sounds was .. 0·542″
Difference, or time of its transmis. thro' the metal .. 0·037″

We do not pretend to give this small difference as exact, since the slightest errour would have a considerable influence on it, but it proves, that the transmission was not absolutely instantaneous.

2d set of experiments, on a

The second set of experiments was made by Messrs. Bouvard and Malus on twice the former number of cylinders, or a length of 394·55 met. [431·184 yds]. At this distance the time of transmission through air would be 1·158″ by calculation, supposing the temperature still 11° [51·8° F.]. The interval between the two sounds, deduced from 64 experiments, was found to be 0·81″. The difference therefore, or 0·348″, was the time of transmission through the solid. This appears much too great, if we compare it with the preceding experiments, and on those that follow, which were made on nearly triple the length. The latter would not permit us to suppose a longer time than 0·125″ for the transmission through the solid, which would give an errour of 0·223″ in the observation. But, beside that it is extremely difficult to answer for such quantities, when the instant of observation does not coincide exactly with a beat of the watch, it must be remarked, that the whole length of the pipe might be far from being at the same temperature, which might occasion currents of air, that would influence the velocity of the sound. For instance, in the present case, if we were to admit the transmission of sound through air as it results from the observations of the chronometer made by Messrs. Martin and Bouvard at the points of departure and arrival, it would be found equal only to 1·07″, or 0·088″ less than the truth, which gives 0·26″ for the time of the transmission of the sound through the solid; and the excess of this result over those that follow, being no more than 0·135″, is more easily reconcilable with errours of observation.

length of 431 yards.

Interval.

Time of transmission through the solid.

This apparently much too great.

Finally, the experiments now to be related were made by Mr. Martin and myself, on a series of 376 cylinders, which, with their joints, formed a length of 951·25 m. [1039·575 yds] of which the joints alone occupy 5·61 m. [6·131 yds]. I satisfied myself at different times, and by more than 200 experiments, either with the hammer or the bell, that the interval between the two sounds transmitted by the metal and by the air, was exactly 2·5″; and I found no sensible variation in this quantity. I made Mr. Martin observe the interval also, without letting him know my results, and he found the same. Now, at the distance of 951·25 met. [1039·575 yds], the temperature being 11° [51·8° F.], the

3d set of experiments, on a length of 1040 yards nearly.

Interval.

time of transmission of the sound through the air would be from calculation 2·79″: and if we substract from this 2·5″, the interval observed between the two sounds, there will remain 0·29″ for the time of transmission through the metal to this distance. From the care with which I repeated these observations, and from the exact coincidence of the five beats of the half-second chronometer with the interval between the two sounds, I believe, that this result may be considered as a very near approximation.

Velocity calculated.

Still however it may be objected, that the velocity of the sound in air deduced from calculation might differ a little from what really took place in the pipe, owing to variation of temperature. This would leave some uncertainty with respect to the result, and particularly as to the precise quantity. I sought therefore to verify it directly in another way, and accomplished it as I shall relate.

As this indirect method might be questioned,

I stationed Mr. Martin at one extremity of the pipe with a half-second watch, while I remained at the other with a similar watch, which was carefully compared with the former at the beginning and end of the experiments: though this comparison could have no influence on the results, as will soon appear. When Mr. Martin's watch was at 0″ or 30″, he struck with a hammer on the last cylinder, near which he was stationed: and when my watch was at 15″ or 45″, I answered him by a similar stroke. We each watched the arrival of the sound transmitted to us, and noted down the time. We were very attentive to strike precisely at the appointed second; and this, with a little practice, we could readily do, as the series of our observations will show. Now, whatever the difference of the watches might be; and even if it were variable, provided there was no sensible change in 30″; it would be reduced to nothing by taking the mean of two consecutive observations, and the result would be independent of it. For, let us suppose the first watch to be the quantity r before the second, and put p for the time in which the sound is transmitted by the solid body. When the first observer strikes on his watch at 0″, the other reads on his 0″ — r; and consequently $p - r$ indicates, before or after 0″, the time at which he hears the

the velocity was measured directly.

sound. On the other hand, when the second observer strikes at 30″, the first observer reads 30″ + r; and consequently $p + r$ indicates, beyond 30″, the time in which the sound is transmitted to him. The quantities $p - r$ and $p + r$ therefore are given by these isochronous observations; and half their sum immediately shows the time of transmission p, independant of the differences between the watches, and more exactly than by direct observation.

In the experiments I made, the series of the quantities $p - r$ and $p + r$ were as in the following table.

	$p - r$	$p + r$	Sum, or value of $2p$.
1st series, from $0^h\ 52'$ to $0^h\ 59'$	−2″	+ 2·5″	0·5″
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
2d series, from $1^h\ 27'$ to $1^h\ 32'$	2·8	3·5	0·7
	2·9	3·5	0·6
	3	3·5	0·5
	2·9	3·5	0·6
	3	3·5	0·5
	3	3·5	0·5
	3	3·5	0·5
	2·9	3·5	0·6
	3	3·5	0·5
	3	3·5	0·5
	3·1	3·4	0·4
Mean value of $2p$.			0·52
Value of p................			0·26

This differs only 0·03″ from what we found above from the difference of the transmissions; but the last method, as it gives double the quantity to be deduced, deserves the preference.

This nearly agrees with the last calculation.

If we add 0·26″, the time of transmission through the solid, to the difference 2·5″ constantly observed between the arrival of the two sounds, we shall have the whole time of the transmission through the air equal to 2·76″. This time, calculated from the length of the pipe, would have been 2·79″, as has just been seen; and the agreement between these numbers, which differ only 0·03″, appears calculated to inspire some confidence in the results.

Velocity in cast iron more than 10 times as great as in air.

The time of transmission through the metal being 0·26″, while that through air is 2·79″, it follows, that the transmission of sound through cast iron is 10·5 times as quick as through air. If this estimation be not sufficiently exact to determine with precision the ratio of the velocities, it is at least enough to show of what kind this ratio is, and what idea we ought to form of it.

Other phenomena observed.

In making these experiments we had an opportunity of observing several phenomena worthy of remark with respect to the power with which sounds, even the faintest, are preserved and transmitted in tubes, to distances at which we could scarcely suppose they would be perceptible.

Conversation easy through a pipe of 215 yards.

In our first experiments at the distance of 197 met. [215 yds.] we heard each other so well through the length of the pipe, that it was an inconvenience in the commencement, as the slightest noise was transmitted from one extremity to the other. It was not necessary to speak into the pipe to be heard, as common conversation two yards from the end was transmitted through it clearly; and in writing down my observations I asked Mr. Martin what it was o'clock by his watch, as I would have done a person only two paces from me. This mode of conversing with an invisible neighbour is so singular, that we cannot avoid being surprised, even though acquainted with the cause.

Speaking loud heard 431 yards.

In the experiments made by Messrs. Malus and Bouvard at the distance of 395 m. [431 yds.] they still heard each other, but with much more difficulty. It was necessary to speak very loud, and frequently to desire a repetition of what had been said. Finally, in the last experiment, which we tried at first together on a total length of 951 m. [1040 yds.], the voice was scarcely to be heard when shouting as loud as possible. The sounds of the bell and of the stroke of the hammer were no longer audible through the air.

At 1040 yds. loud shouting scarcely audible, and the sound of the bell or the hammer not air. The sound through the metal alone was perceptibly transmitted. Lastly, though we could still hear the sound of the voice, it was not sufficiently clear for us to distinguish words, or to transmit the necessary information after our observations. From the great difficulty, which Messrs. Malus and Bouvard had already experienced at a much shorter distance, we were all inclined to suppose, that we had attained a distance, at which the human voice, even the loudest, ceases to be distinguishable in pipes.

at all through the air.

However, the extreme facility with which we heard each other at 200 metres seemed to me to render so great a diminution altogether inexplicable. Besides, in the mathematical theory of the motion of air we find nothing to indicate, that sound should be diminished in cylindrical pipes. It appears on the contrary, that it ought to be transmitted to an indefinite distance with the same intensity, deducting merely the diminution, that the friction of the air against the pipe might perhaps produce. To decide the question, and know positively whether sound were weakened in such an extraordinary degree, I resolved to remove or diminish all the causes of foreign and neighbouring noises, that might drown the sound I sought to hear. I went to the place of experiment only with Mr. Martin and two intelligent workmen, and chose for these experiments the stillest hours of the night, those from one to four in the morning.

But this improbable from the 1st experiments,

and from theory.

The experiments repeated in the dead of night,

I then discovered, that my conjectures were well founded. We not only heard the two sounds of the hammer and bell so distinctly as to observe the intervals such as I have reported them; but even the lowest voice was heard so as perfectly to distinguish the words, and to keep up a conversation on all the objects of the experiments. I wished to determine the point at which the voice ceases to be audible, but could not accomplish it: words spoken as low as when we whisper a secret in another's ear were heard and understood; so that not to be heard there was but one resource, that of not speaking at all.

when not only the sounds of the bell and hammer, but the lowest whisper was heard.

From this experiment there can be no doubt, that words may be transmitted so as to be distinctly heard at a more considerable distance. Between a question and answer the interval was not greater, than was necessary for the transmission of sound. For Mr. Martin and me, at the distance of 951 m. [1040 yds.], this time was about 5·58.″

Grave and acute sounds have equal velocity.

We also ascertained anew, that grave and acute sounds are transmitted with equal velocity, which is agreeable to theory, and has been several times observed. Tunes on the flute, played at one extremity of the pipe, were transmitted to the other without any alteration in the intervals of the different intonations. It appeared to me only, that the very high notes were not heard so well as the low notes; and sometimes, when they were extremely high, I lost them entirely; though I heard others that were lower, which, from the nature of the tune, I knew to be weaker than the former*.

Playing on the flute.

Echo of the voice returned repeatedly to the speaker,

I also observed, that, in speaking through the pipe, I heard my own voice repeated by several echoes, which succeeded each other at exactly equal intervals. In our last experiment I counted no less than six, about 0·5′ distant from each other. The last returned after a little less than 3′; that is, in the time requisite for the transmission of the sound to the other end of the pipe. These phenomena occurred equally at each extremity of the pipe, when we spoke into it. Of this I satisfied myself by requesting Mr. Martin, through the pipe, to observe them, without communicating to him my results: and his, which he reported to me immediately in the same way, were perfectly similar. The number of echoes and their intervals were the same, and the total of the time was the same also; but the person who is spoken to never hears but one sound.

but the sound at the other end single

Detonations.

Lastly, detonations capable of producing a considerable agitation in the air were transmitted to the other end of the pipe with an intensity proportional to their strength. Reports of a pistol fired at one end occasioned a considerable explosion at the other. The air was driven out of the pipe

A pistol fired at one end blows out a candle at the other.

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* Since this paper was read, I have found, that the person who played the flute, having very weak lungs, could with difficulty bring out the high notes, and was frequently obliged to skip them entirely. It was very natural therefore, that I should not hear them: but I have thought proper to let my first account remain, that the reader may see I reported faithfully the smallest particular; and that my veracity in this circumstance may confirm the other results I observed.

with sufficient force to give the hand a smart blow, to drive light substances out of it to the distance of half a yard, and to extinguish a candle, though it was 950 m. [1039 yds.] distant from the place where the pistol was fired.

V.

Observations and Experiments on Pus. By GEORGE PEARSON, *M. D. F. R. S.*

(Concluded from p. 27.)

SECTION VII. *Conclusions.*

General conclusions.

THE statement of the properties of pus in the foregoing inquiry I hope will be found to be true; and I submit to the judgment of others whether or no the following inferences are legitimately established.

Pus consists of three distinct substances.

1. That this fluid essentially consists of three distinct substances, viz. 1. An animal oxide, which, among other properties, is distinguished by its being white, opaque, smooth, of the form of fine curdy particles in water; not dissoluble in less than 1000 cold waters; not coagulable into one mass like serum of blood by caloric, alcohol, &c.; only rendered more curdy by water from 160° to 170°; but readily diffusible.—2. A limpid fluid resembling serum of blood in its impregnations, and in its coagulability by caloric, alcohol, &c.; in which the opaque oxide is diffusible but not dissoluble, and which is specifically lighter than that oxide.—3. Innumerable spherical particles visible only by the microscope in this opaque oxide, and in small number in the limpid fluid; not coagulable by any temperature to which hitherto exposed, and not destructible by many things which combine or destroy the opaque oxide; and these globules are specifically heavier than water*.

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* My obligingly attentive pupils, Mr. BURTON, and Mr. STANSFELD, house-surgeons of the Lock hospital, collected for me a sufficient quantity of gonorrheal matter to determine, that it consisted of the three ingredients here stated.

2. That

Visible curdy masses.

2. That the *visible* curdy masses, as well as the fibrous or leafy parts, almost always contained in smaller or larger quantities in pus, may be considered as self-coagulated lymph, which in its fluid state is secreted without having the state of aggregation produced in it like that of the *essential* opaque oxide of pus.—Sect. VII, 1.

Red or dark colour of pus.

3. That the reddish, the blackish, and the dark brown colour of pus depends upon the red part of the blood effused or secreted from the same vessels, or from contiguous ones which secrete pus.

Irregular masses.

4. That on some occasions the clotty and irregularly figured masses found in the pus may depend upon disorganization or breach of the contiguous solid parts.

Fœtor.

5. That whenever pus is fœtid to the smell, a portion of it is in the state of putrefactive fermentation, which may be removed by ablutions with water.

Adventitious contagious matters.

6. That there are certain adventitious matters liable to be contained in pus not hitherto rendered palpable to the senses, but known by their effects in exciting contagious diseases; such as small-pox, syphilis, &c. These matters are produced by a specific action in the secretory organs of pus, by such matters themselves either contained in the circulating blood, or on the secreting surface.

Secretion of pus from the blood.

7. That the *essential* substances of which pus consists, as well as some of the adventitious ones (Sect. VII, 1, 2, 3, 6), are separated from the blood by a peculiar organization belonging, or attached to the blood-vessels: which organs of separation or secretion are not only excited to the action which produces pus in descased states, but they are evidently influenced by the states of other distant organs of the animal œconomy; hence many varieties in the properties of the purulent matter.

Sources of the differences of pus.

8. That the varieties of purulent matter relate to differences of *quantity*—the proportion of the essential substances (1)—and the adventitious parts (2, 3, 4, 5, 6,). The *cream-like* pus consisting of almost purely the opaque oxide and limpid liquid (I, 1, 2,). The *curdy* containing a large proportion of coagulated lymph, or broken down solids. The *serous* abounding in limpid fluid. The *viscid* depending

upon the coagulation, and perhaps, inspissation, by union of neutral salts with the opaque oxide.

Self-coagulated lymph of the blood and serum separated by secretion.

9. That as the essential parts are secreted in a limpid state, but presently become opaque, owing to a large proportion spontaneously coagulating, and thus becoming the opaque oxide, mixed with the serous liquid, and innumerable spherical particles (Sect. VII, I, 1, 2, 3), it seems reasonable to infer, that these matters are the self-coagulated lymph of the blood and serum, separated by the secretory organs; which act of secretion determines the subsequent state of aggregation of pus, and the globules are at the same time formed analogously to their formation by other secretory organs. How far they are those of the blood altered by secretion may be determined hereafter. It is a collateral proof of this inference, that very thick pus affords from one sixth to one seventh of exsiccated brittle residue, which, as I have found, is nearly the same proportion afforded on the exsiccation of the buffy coat of inflamed blood; while very thin pus affords on exsiccation from one eighth to one eleventh of brittle residue, which is the proportion to be expected from a mixture of serum of blood and self-coagulated lymph, as I have ascertained.

Saline and earthy ingredients.

10. That the constant impregnating saline and earthy ingredients of pus are dissolved in the serous fluid; and are all separable along with the serum, by ablutions with water, from the opaque oxide (1), except a portion of the phosphate of lime. These impregnations are the same as those of serum of blood, and of expectorated mucous matter, *viz.* muriate of soda; potash neutralized by animal matter or a destructible acid; phosphate of lime; ammonia neutralized probably by phosphoric acid; with a sulphate, and traces of some other matters mentioned in my former paper. The proportion of these impregnating substances is as the proportion of limpid or serous coagulable fluid, and of course inversely as the proportion of the opaque oxide of pus; but it varies in different cases in given proportions of this oxide, and the limpid fluid. In general, if not always, a given quantity of pus contains a smaller proportion of saline matters than an equal given quantity of expectorated mucous matter, but a given quantity of the limpid coagulated fluid

contains a greater proportion of saline matters than an equal given quantity of serum of blood. Hence the thicker the pus the less irritation to the sore which secretes it, and commonly the less the inflammatory or other action of the secreting surface. In different cases, however, the proportion of impregnating saline substances to one another is liable to vary, especially that of phosphate of lime; hence, though rarely, calculi occur of this substance in the cavity of the abscess*. Hence too the exsiccated pus is liable to become soft and moist, from the proportion of neutralized potash being greater than usual; and even deliquescence sometimes occurs of the exsiccated limpid fluid.

Calculi in abscesses.

Different secretions from the same organs in different states.

12. That the same organs, according to their different states, secrete from the blood merely water impregnated with the saline substances of the serum of blood; also this fluid containing various proportions of coagulable matter like that of serum of blood; and serous fluid with self-coagulable lymph, which affords curdy masses: likewise this serous fluid, together with this matter which coagulates of itself after secretion, highly impregnated with invisibly small particles, in such a state of aggregation, as to constitute the thick opaque fluid called pus—which states of the secretory organs are generally attended with inflammatory action, but frequently also without any symptoms of such action.

Consistence of pus.

13. That beside the consistence of pus depending upon the proportion of serous limpid liquid, and opaque matter, it also probably depends upon the mode and state of coagulation of the matter which affords this opaque part; analogously to the different states of consistence of the coagulated blood itself, according to the different conditions of the animal œconomy.

Distinction of pus from other matters.

According to the above inferences, I trust, a distinct and definite notion of the substance to be considered as pus is exhibited

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Stones in the lungs.

* On examining the lungs of a patient who died of pulmonary consumption, concretions were found in a large vomica from the size of mustard seed to a pepper corn, which Dr. E. N. BANCROFT reserved for my inquiry. I found they consisted chiefly of phosphate of lime, with an unusually small proportion of animal matter. In another patient of Dr. NEVINSON, matter was coughed up, consisting chiefly of phosphate of lime and animal matter, nearly one of the former to three of the latter.

exhibited; and I do not comment on the different results of experiment and conclusions of other writers, because future observers only can determine the truth. What is and what is not pus will now readily be acertained by a few easy experiments; by the obvious properties; and by the consideration of the source of the matter in question: provided, however, that it be unmixed with certain other matters, by which disguise is produced. As already observed it is in pulmonic diseases that the ambiguity occurs; and physicians lay very considerable stress upon the nature of expectorated matter in their practice and reasoning; I shall therefore endeavour to elucidate the subject by remarks on the puriform matter expectorated in different cases.

Puriform matter expectorated

1. An abscess occasioned by acute inflammation not only of a pleurisy, and peripneumony, but of other diseases which have not the symptoms of any one which has received a designation. Here there ought to be no doubt; for the matter which is coughed up suddenly and abundantly on the bursting of the abscess is evidently pus with little mucus. Such matter consists of the essential ingredients of pus, (Sect. VII, 1,) with generally the adventitious substances, (Sect. VII, 2, 3, 4,) *viz.* coagulated lymph, membranous or fibrous parts, and a small proportion of the red part of blood.

from an abscess after acute inflammation;

2. Purulent expectoration from the rupture of abscesses, or vomicæ of suppurated tubercles. In such cases there has been a chronical cough with viscid sputum, commonly in persons of an advanced age. After this long continued disease, an abundant expectoration of quite a different kind from the former suddenly comes on; by which the patient often dies very speedily; sometimes immediately, being seemingly choaked. This kind of matter evidently consists chiefly of the essential ingredients of pus (Sect. VII, 1,) with not only the adventitious substances, *viz.* clots of self-coagulated lymph, and sometimes the red part of blood, but also masses, which are apparently the broken down solid parts, the cellular membrane, the vessels, and substance of the tubercles, in a disorganized state. The sufferer often says, such matter tastes sweet. The mucus is here in too small a proportion, and not intimately mixed, to occasion disguise.

from the rupture of abscesses of suppurated tubercles;

3. In the bronchitis, or inflammatory affection of the air

in inflam-

tubes

matory affection of the air-tubes.

tubes, the membrane remaining entire, attending various diseases, e. g. the measles, a fever with a cold, various continued fevers, an expectoration of thin creamlike matter occurs, at first gradually, but at last in great quantities, continuing for a week or more. Although mucus is usually coughed up with this puriform substance, the two things generally remain in distinctly large masses. With little skill the opaque or puriform fluid may be collected separately from the mucous matter. It will be found to consist almost purely of the three essential constituents of pus (Sect, VII, 1,) there being seldom any adventitious substances.

Muco-purulent matter.

4. Muco-purulent, or commixed expectorated matter. This kind is perhaps of the most frequent occurrence. It is that which many physicians know not how to designate, some consider it to be pus, and others to be mucous matter. This contrariety of opinion arises from the want of definite notions of pus and mucus. Hence the parties are not able to perceive, that in this kind of sputum exist many of the properties of pus, and also of mucus. I have described it in my former paper on expectorated matter. Phil. Trans. 1809, P. II, p. 317*, under the denomination of *opaque ropy matter*, the third kind. I feel no degradation in finding it necessary to confess, that a better acquaintance with the properties of pus has taught me, that I was in an errour, in considering this kind of expectorated matter to differ from other sorts merely in the proportion, and not in the kinds, of constituent parts. It now appears that the sputum in question possesses such properties as might be predicted to exist, from the known properties of pus and mucus separately, in case these two substances should be intimately commixed. Accordingly, the opacity; the straw colour; the greater density than mucus; the great globularity under the microscope; the greater proportion of residue on evaporation to dryness, than from mucus; the milky liquid on heating this matter; the milkiness on agitation in cold water; are properties of pus. But the great viscidity, yet not increased by neutral salts; the less opacity than pus; the less globularity than pus; the smaller porprotion of exsiccated residue than

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* Journal, vol. XXV, p. 219.

from pus; the moisture, or greater moisture on the exposure of the brittle residue to air, than from that of pus; the more difficult diffusibility through cold water, and less degree of milkiness than from pus: the great proportion of leafy or fibrous masses on agitation in a very large quantity of cold water; the speedy putrescency; are properties of mucus. The mode of coagulation by caloric at 160° and upwards is such as might be expected from the commixture, *viz*: in large masses of curd in a milky liquid, instead of into one uniform mass like pus, or into small curdy masses in a very large proportion of a whey coloured liquid, like mucous sputum. Thick pus affords on evaporation to brittleness, $\frac{1}{7}$ or $\frac{1}{8}$ residue; and transparent sputum of the consistence of jelly, gives about $\frac{1}{15}$ or $\frac{1}{20}$ of such residue: but this opaque matter under inquiry, affords $\frac{1}{10}$ or $\frac{1}{12}$ of brittle residue, according to the proportion of the two substances. I could not separate the supposed pus and mucus from one another, to exhibit them distinctly by water, or by any other means, on account, as I conceive, of the intimate diffusion through one another, and their mutual cohesion. But on evaporating the milky water, produced by agitating this sputum in it, or by letting it stand to collect the sediment, little else besides a mere congeries of globules seen under the microscope was thus obtained. For the same reason, on standing, a serous liquid like that of pus (Sect. VII, 1) does not separate, or only partially, from the opaque part, so as to render it possible by ablution, to collect this coagulable liquid like that of pus: and the greater proportion of water, belonging to the mucus, occasions the coagulation by caloric, to afford only a milky liquid, instead of a uniform mass of curd.

From secretion without ulceration.

This kind of sputum, consistently with the phenomena, must be produced by secretion from the bronchial membrane in its entire state, and not by ulceration or abscess. For it is secreted in many cases, at the rate of a pint or more in each 24 hours, for weeks and months successively, and for 20 or more successive winters. Also many persons recover their good health after this secretion, and it is the usual termination favourably of pneumonia, bronchitis, &c. It is produced by any disease of great irritation of the lungs; as I have found from

Broken wind.

from ossification of the bronchial or pulmonary arteries: from calculi: from broken wind, or rupture of air cells, &c.*

Secreted in other cases, and from the nose.

It is secreted also in consequence of irritation of the bronchial membrane by tubercles, vomicæ, water in the cavities of the chest, &c. The same kind of matter is secreted from the nose on the decline of a common severe coryza in many cases.

Sometimes indicates death, sometimes recovery.

It appears then, that this kind of matter is a symptom of the most fatal, as well as harmless diseases—it is a symptom in one case of the progress of disease to death, and in another of the termination in health, by being seemingly a critical discharge. Perhaps, if these facts had been observed and considered, numerous mistakes in prognostics would have been avoided, and better practice have been employed; because the nature of diseases would have been rightly understood. From this representation it is plain, that a just opinion cannot be given merely from the examination of the sputum, without considering the disease by which it is produced, or of which it is a symptom.

The proportion must also be considered of the pus and mucus in sputum: it may be estimated, by attending to the properties of each, as above stated.

Secretion of muco-purulent matter.

Such a compound as the present scarcely is produced in any other part, but in the bronchial, and mucous membrane of the nose, because of the abundant secretion of mucus from these membranes. And when it is conceived, that both pus and mucus are secreted in a limpid state, from the same or at least contiguous organs, where they first intimately commix, and then become inspissated; it will appear reasonable, that they cannot be readily, or at all completely separated again from one another. There is indeed, in these cases, no necessity for the admission of the secretion of the limpid fluid of pus of abscesses (Sect. VII, 1); for it appears to me not unjust to consider mucus to be nothing more than the serum of blood, altered in its composition and proportion of water, so as to produce a viscid texture. The secretory organs of the mucous membrane, by virtue of their peculiar power, separate from the blood, in health, the mucus as above said, with some globules, and also a small proportion of the self-coagulable lymph; which appears, on agitating mucus in a large proportion of cold water, in the form of leafy and fibrous masses[*]. The same secretory organs, it is easily conceivable, may, in a diseased state, be excited to separate also self-coagulable matter from the blood, with more globules, in such a state as to become pus. Hence, such a commixture of the two substances must correspond to the opaque, viscid, expectorated sputum, of which I am writing.

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* I believe this state of the lungs to have been first ascertained in broken-winded horses, by Mr. Colman.

If I thought farther reasoning proper, it would be manifest, that all the phenomena, both in health and disease, belonging to the various kinds of sputum, consist with the theory above delivered.

VI.

Description of a Tachometer, or an Instrument to ascertain the Velocities of Machinery: by Mr. BRYAN DONKIN, *of Fort Place, Bermondsey*[†].

Advantageous to ascertain the velocity of machinery.

IN the employment of machinery it is evidently of great importance, to be provided with an easy and ready method for discovering at all times, whether the motion of the machine is quicker or slower than what is known to be best adapted for the object in view. This advantage, it is hoped, may be derived from the tachometer; for it is an instrument which requires only to be adjusted once for all to any particular machine, and then it will always be ready without the help of calculation or of a time-piece, to indicate instantly upon inspection the slightest excess or defect in the actual velocity.

An instrument for this purpose described.

A front view of the tachometer is represented in fig. 1, and a side view in fig. 2, of Pl. III. X Y Z, fig. 1, is the vertical section of a wooden cup, made of box, which is drawn in elevation at X, fig. 2. The whiter parts of the section, in fig. 1, represent what is solid, and the dark parts what is hollow. This cup is filled with mercury up to the level L L, fig. 1. Into the mercury is immersed the lower part of the upright glass tube A B, which is filled with coloured spirits of wine, and open at both ends, so that some of the mercury in the cup enters at the lower orifice, and when every thing is at rest, supports a long column of spirits, as represented in the figure. The bottom of the cup is fastened by a screw to a short vertical spindle D, so that when the spindle is whirled round, the cup, (the figure of which is a solid of revolution) revolves at the same time round its axis, which coincides with that of the spindle.

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[*] Serum of blood appears always to contain self-coagulable lymph, which is deposited on standing; and this appearance led Gaber, Pringle, and Cullen, into the erroneous opinion of this deposit being pus itself.

[†] Trans. of the Soc. of Arts, vol. XXVIII, p. 185. The gold medal was voted to Mr. Donkin for this invention.

In consequence of this rotation, the mercury in the cup acquires a centrifugal force, by which its particles are thrown outwards, and that with the greater intensity, according as they are more distant from the axis, and according as the angular velocity is greater. Hence, on account of its fluidity, the mercury rises higher and higher as it recedes from the axis, and consequently sinks in the middle of the cup; this elevation at the sides, and consequent depression in the middle, increasing always with the velocity of rotation. Now the mercury in the tube, though it does not revolve with the cup, cannot continue higher than the mercury immediately surrounding it, nor indeed so high, on account of the superincumbent column of spirits. Thus the mercury in the tube will sink, and consequently the spirits also; but as that part of the tube which is within the cup is much wider than the part above it, the depression of the spirits will be much greater than that of the mercury, being in the same proportion in which the square of the larger diameter exceeds the square of the smaller.

Method of using it.

Let us now suppose, that, by means of a cord passing round a small pulley F, and the wheel G, or H, or in any other convenient way, the spindle D is connected with the machine, the velocity of which is to be ascertained. In forming this connection, we must be careful to arrange matters so, that, when the machine is moving at its quickest rate, the angular velocity of the cup shall not be so great as to depress the spirits below C into the wider part of the tube. We are also, as in the figure, to have a scale of inches and tenths applied to A C, the upper and narrower part of the tube, the numeration being carried downward from zero, which is to be placed at the point to which the column of spirits rises when the cup is at rest.

Then the instrument will be adjusted, if we mark on the scale the point to which the column of spirits is depressed, when the machine is moving with the velocity required. But, as in many cases, and particularly in steam-engines, there is a continued oscillation of velocity, in these cases we have to note the two points between which the column oscillates during the most advantageous movement of the machine.

Correction for temperature.

Here it is proper to observe, that the height of the column of spirits will vary with the temperature, when other circumstances are the same. On this account the scale ought to be movable; so that, by slipping it upwards or downwards, the zero may be placed at the point to which the column reaches when the cup is at rest; and thus the instrument may be adjusted to the particular temperature with the utmost facility, and with sufficient precision. The essential parts of the tachometer have now been mentioned, as well as the method of adjustment; but certain circumstances remain to be stated.

Precautions.

The form of the cup is adapted to render a smaller quantity of mercury sufficient, than what must have been employed either with a cylindrical or hemispherical vessel. In every case two precautions are necessary to be observed: First, That, when the cup is revolving with its greatest velocity, the mercury in the middle shall not sink so low as to allow any of the spirits in the tube to escape from the lower orifice; and that the mercury, when most distant from the axis, shall not be thrown out of the cup. Secondly, That, when the cup is at rest, the mercury shall rise so high above the lower end of the tube, that it may support a column of spirits of the proper length.

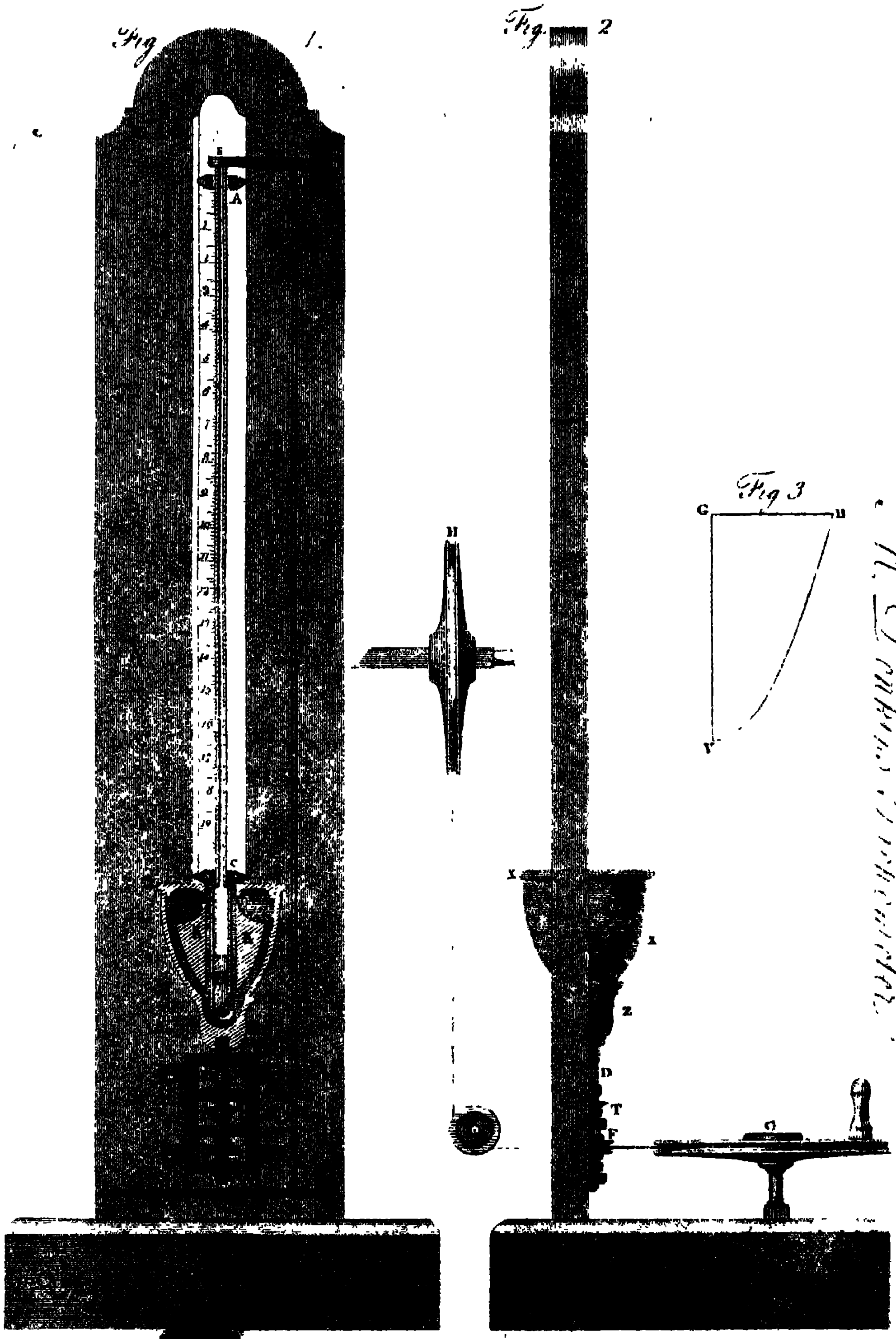
Now in order that the quantity of mercury, consistent with these conditions, may be reduced to its minimum, it is necessary—first, that if M M, fig. 1, is the level of the mercury

cury at the axis when the cup is revolving with the greatest velocity, the upper part M M X Y of the cup should be of such a form, as to have the sides covered only with a thin film of the fluid; and secondly, that for the purpose of raising the small quantity of mercury to the level L L, which may support a proper height of spirits when the cup is at rest; the cavity of the cup should be in a great measure occupied by the block K K, having a cylindrical perforation in the middle of it for the immersion of the tube, and leaving sufficient room within and around it for the mercury to move freely both along the sides of the tube and of the vessel.

The block K K is preserved in its proper position in the cup or vessel X Y Z, by means of three narrow projecting slips or ribs placed at equal distances round it, and is kept from rising or floating upon the mercury by two or three small iron or steel pins inserted into the underside of the cover, near the aperture through which the tube passes.

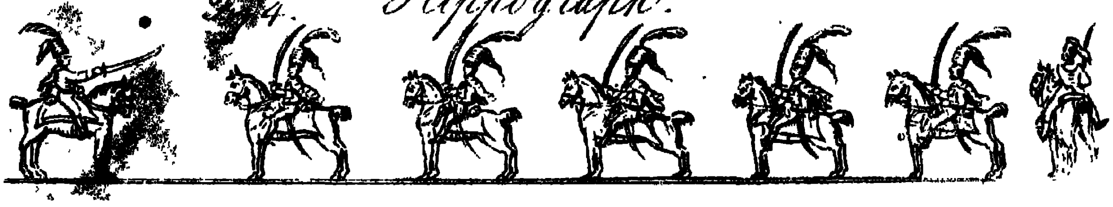
Form of the cup.

It would be extremely difficult, however, nor is it by any means important, to give to the cup the exact form, which would reduce the quantity of mercury to its minimum; but we shall have a sufficient approximation, which may be executed with great precision, if the part of the cup above M M is made a parabolic conoid, the vertex of the generating parabola being at that point of the axis to which the mercury sinks at its lowest depression, and the dimensions of the parabola being determined in the following manner: Let V G, fig. 3, represent the axis of the cup, and V the point, to which the mercury sinks at its lowest depression; at any point G above V, draw G H perpendicular to V G; let n be the number of revolutions, which the cup is to perform in 1″ at its quickest motion; let v be the number of inches, which a body would describe uniformly in 1″, with the velocity acquired in falling from rest, through a height = to G V; and make G H $= \frac{v}{3.14\,n}$. Then, the parabola to be determined is that which has v for its vertex, V G for its axis, and G H for its ordinate at G. The cup has a lid to prevent the mercury from being thrown out of it, an event which would take place with a very moderate velocity of



Mr. Donkin's Tachometer.

Fig. 4. Hippograph.



of rotation, unless the sides were raised to an inconvenient height; but the lid, by obstructing the elevation at the sides of the cup, will diminish the depression in the middle, and consequently the depression of spirits in the tube: on this account a cavity is formed in the block immediately above the level L L, where the mercury stands when the cup is at rest; and thus a receptacle is given to the fluid which would otherwise disturb the centrifugal force, and impair the sensibility of the instrument.

Curve at the bottom of the tube.

It will be observed, that the lower orifice of the tube is turned upwards. By this means, after the tube has been filled with spirits by suction, and its upper orifice stopped with the finger, it may easily be conveyed to the cup and immersed in the quicksilver without any danger of the spirits escaping, a circumstance which otherwise it would be be extremely difficult to prevent, since no part of the tube can be made capillary, consistently with that free passage to the fluids, which is essentially necessary to the operation of the instrument.

Method of setting the instrument in motion.

We have next to attend to the method of putting the tachometer in motion, whenever we wish to examine the velocity of the machine. The pulley F, which is continually whirling during the motion of the machine, has no connection whatever with the cup, so long as the lever Q R is left to itself. But when this lever is raised, the hollow cone T, which is attached to the pulley and whirls along with it, is also raised, and embracing a solid cone on the spindle of the cup, communicates the rotation by friction. When our observation is made, we have only to allow the lever to drop by its own weight, and the two cones will be disengaged, and the cup remain at rest.

The lever Q R is connected by a vertical rod to another lever S, having at the extremity S a valve, which, when the lever Q R is raised, and the tachometer is in motion, is lifted up from the top of the tube, so as to admit the external air upon the depression of the spirits; on the other hand, when the lever Q R falls, and the cup is at rest, the valve at S closes the tube, and prevents the spirits from being wasted by evaporation.

Increase of the

It is lastly to be remarked, that both the sensibility and the sensibility and range of the instrument.

the range of the instrument may be infinitely increased; for, on the one hand, by enlarging the proportion between the diameters of the wide and narrow parts of the tube, we enlarge in a much higher proportion the extent of scale corresponding to any given variation of velocity: and on the other hand, by deepening the cup so as to admit when it is at rest a greater height of mercury above the lower end of the tube, we lengthen the column of spirits which the mercury can support, and consequently enlarge the velocity, which, with any given sensibility of the instrument, is requisite to depress the spirits to the bottom of the scale.

Applicable to delicate experiments.

Hence the tachometer is capable of being employed in very delicate philosophical experiments, more especially as a scale might be applied to it, indicating equal increments of velocity. But in the present account it is merely intended to state how it may be adapted to detect in machinery every deviation from the most advantageous movement.

VII.

A Mode of conveying Intelligence from a reconnoitring Party. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

Mode of conveying intelligence by a reconnoitring party.

I Herewith send you a model, which I denominate a *Hipograph*, and which appears to me likely to be of use in the march of troops, &c.

It may consist of any number of men and officers, but I conceive an officer and six men quite sufficient. The use it seems most adapted to is, when a mountain or high ground is in front, and it is wished by the commanding officer to know what may be on the other side, by dispatching such a number of men intelligence can be at once conveyed by changing the front of one or more men to express numbers, or permanent signals, as agreed on, as the boards of a telegraph; and by the officer placing himself on either flank, centre, or rear, the numbers would be quadrupled. I know by experience it may be distinguished at a great distance. Should you think this worthy of notice, it will be a satisfaction to,

Sir, your obedient servant,

August, 1811. H. I. B.

The model consists of little tin casts of six horse soldiers and one officer, see Pl. III, fig. 4. These are placed on a slip of wood, and each is movable on a pivot, so that it may be turned into any position.

VIII.

Description of a Machine for separating Iron Filings from their Mixture with other Metals: by Mr. J. D. Ross, *Princes Street, Soho*[*].

SIR,

I Hope you will be pleased to lay before the gentlemen of the Society of Arts &c. the model of a machine, which I have invented to separate iron-filings, turnings, &c., from those of brass or finer metals, in place of the slow and tedious process hitherto employed, which is by a common magnet held in the hand. By my invention many magnets may now be employed at once, combined and attached to a machine on a large scale. The magnetic hammers are so contrived as to take up the iron-filings from the mixture of them with other filings, or metallic particles, placed in the trays or end boxes, and drop them into the receiving box in the centre, which is effected by the alternate motion of a winch-handle, working the two magnetic hammers placed at two angles of a quadrant or anchor. In proportion to the power of the magnets, and to the force of the blow given by the hammers, a great quantity of iron is separated from the brass, by the alternate motion, and dropped into the receiver placed in the centre of the machine.

Machine for separating iron filings from other matters.

I have shown the model to persons engaged in various metallic works*, who give me great encouragement by their signatures and sanction, and I hope it will meet with the Society's approbation.

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[*] Trans. of the Soc. of Arts, &c., vol. XXVIII, p. 206. Five guineas were voted to Mr. Ross for this invention.

I am, Sir,
Your most obedient and humble servant,
J. D. ROSS.

Reference to Mr. Ross's Machine for separating Iron Filings from those of Brass, or other Metals, Figs. 1 *and* 2, Pl. IV.

Description of the machine. A is an axis of brass, and B a handle upon the end of it: C is a piece of brass in form of an anchor, at each end of which a horse-shoe magnet is fixed, in the manner shown at fig. 1, where *c* is the arch of the anchor, and *d* a piece of brass having a hole through it to receive the legs *e e* of the magnet, which is fixed to the arch by a screw *f*, tapped into the arch. The anchor is mounted upon the pivots of the axis A, in a frame E, which encloses it; on the outside of the frame are two blocks of wood, FF, in each of which a hollow or tray is formed to receive the filings which are to be separated from the iron they contain in these hollows. Its mode of operation. The magnets fixed at the ends of the anchor strike upon the filings, and select, by the magnetic attraction, all the iron among them: the anchor is then turned over by the handle B, and the opposite magnet strikes in the other hollow F. At this time the other magnet is just over the axis, and by the jerk of its opposite striking the block F, the iron-filings are shaken off, and fall down on the bottom of the frame, or receiver. In this manner the handle B, being moved backwards and forwards, strikes the magnets alternately in the two blocks F; and at the same time that one strikes, the opposite is cleared from the iron it has picked up by the shock. G is a screen of thin board to prevent the filings being scattered.

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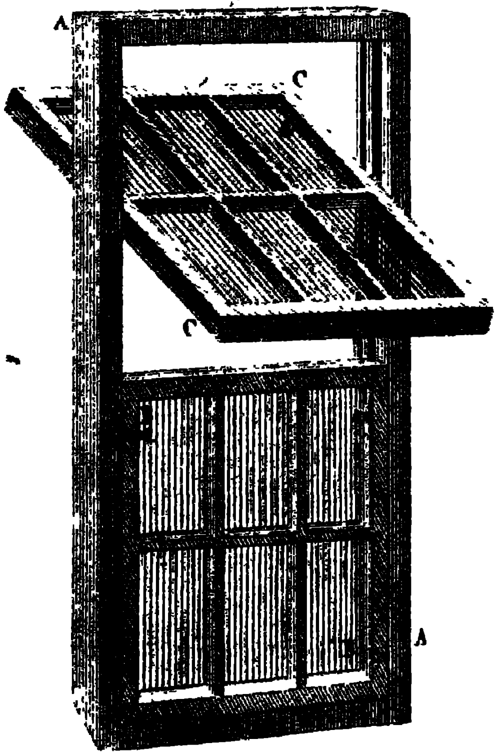
* Eleven different persons certified, that they consider Mr. Ross's invention of a machine for separating iron-filings, turnings, &c., from those of brass or finer metals, as likely to prove extremely useful in various branches of workers in metal.

IX.

Mr G. Marshall's. New Window Sash.

Fig 3.



Mr Roſs's. Machine for separating Iron filings from those of Braſs.

Fig.1.

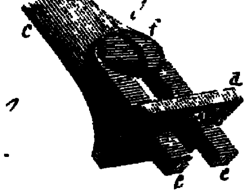
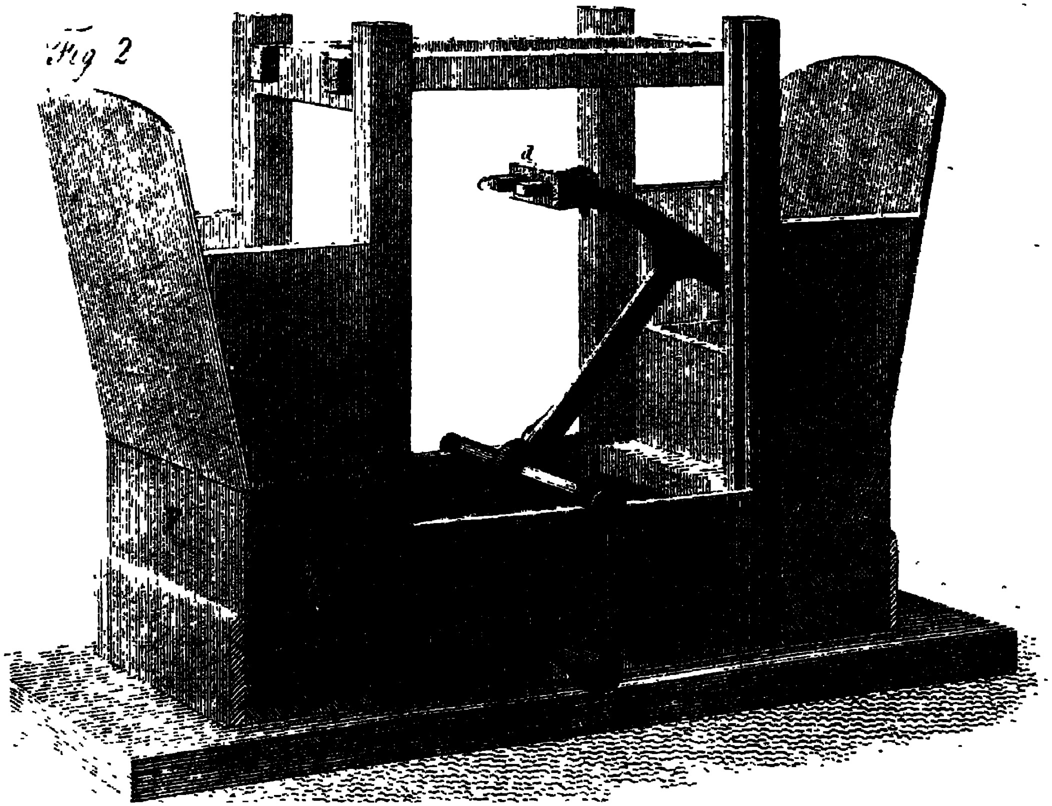


Fig 2



IX.

A new Method of constructing Sash Windows, so as to be cleaned or repaired without the necessity of any Person going on the outside of the House: *by* G. MARSHALL, *No.* 15, *Cecil Court, St. Martin's Lane**.

SIR,

IN consequence of the numerous accidents, which occur from cleaning and painting the outside of windows, I beg leave to submit to the inspection of the Society a model of a sash-window, which, if it meets their approbation, and becomes generally adopted, will, I think, save the life of many a fellow-creature; because the present mode of cleaning or painting the outside of windows is generally done by persons leaning out of the window, or getting upon a plank, or some other convenience made for the purpose, and projecting on the outside of the house; hence, from carelessness and inattention, many fatal accidents have occurred, and the services of many persons lost to their families and the public. One instance of this kind happened about three weeks ago to a man, who was standing on a board cleaning the outside of a window, when, the board giving way, as frequently happens, the man was precipitated, and impaled upon the spikes of the iron pales, which enclosed the area below, whence he was conveyed to the hospital with no hopes of recovery. This unhappy man, I was informed, had a large family depending upon him for subsistence. I was so shocked with the circumstance, that I was not easy till I had made the model, which I thought would be the means of preventing similar accidents. This model I beg leave to lay before the Society, and if it should be so fortunate as to meet with their encouragement, I will receive any donation from them with thankfulness, and have no doubt that it will be found to possess many advantages. In appearance it resembles a common sash, and the upper or lower sheet may be moved up and down in a similar

Accidents from cleaning windows frequent.

Contrivance to prevent this.

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* Trans. of the Soc. of Arts, &c., vol. XXVIII, p. 209. Fifteen guineas were voted to Mr Marshall.

Another advantage.

The expense trifling, either in new or old sashes.

similar manner; beside which, by pushing two small springs back in the upper sheet, and at the same time pulling the sash inwards, you may turn the outside of the sash towards you, into the room, so that it may be easily painted, glazed, or cleaned by a person standing within the room, without the necessity of removing the slips or beadings, by doing which, in the common mode, the glass is frequently broken and the beads lost, left loose, or mismatched, and a considerable expense incurred. By turning the lower sash of my invention in a horizontal or inclining direction, you can look into the street without being wet in rainy weather, or the rain driving into the room and damaging the furniture. Old windows may be altered to act upon this principle, at an expense of twelve shillings per window; and new sashes and frames may be thus made for only six shillings more than the common price.

I remain, Sir,

Your obedient humble Servant,

GEORGE MARSHALL.

Reference to the Delineation of Mr. Marshall's Window-Sash, fig. 3, Pl. IV.

Explanation of the plate.

A A represents the window-frame; B B the lower, and C C the upper sash. The frame A A is fitted with grooves, weights, and pullies, in the usual manner; the fillets on the sash, which enter the grooves, are not made in the same piece with the sash-frame, but fastened thereto by pivots about the middle of the sash; upon these pivots the sash can be turned as at C C, so as to get at the outside without disturbing the fillets or grooves; when the sash is placed vertically, as at B B, two spring-catches at *a a* shoot into and take hold of the sliding fillets, so that in this state the sash slides up or down in the usual manner; but it can be immediately released, and turned inside out, by pushing back the springs, and at the same time pulling the sash inwards; this turns the outside towards the room, so that the sash may easily be painted, glazed, or cleaned on the outside by a person within the room, without removing the beads, which confine the sash to slide up and down vertically; in the common way these beads are frequently broken or misplaced, and cause considerable trouble by being always loose. By inclining the sash on its pivots, the highest point being within the room, the window may be left open in the most severe rain without danger of any entering the room, and a person may look out into the street without being wet.

X.

Observations on the peculiar Appearances of those Meteors commonly called Shooting Stars. In a Letter from Thomas Forster, *Esq.*

To W. NICHOLSON, Esq.

SIR,

Peculiarity in the electric state of the atmosphere.

ONCE more I trouble you with some meteorological observations, which, if you think worthy, I shall be obliged to you to insert in your next. In a former number of your Journal I noticed an apparent peculiarity in the electric state of the atmosphere, during which the action of Mr. De Luc's aerial electroscope was very irregular. The principal circumstances, which characterised such a state of the atmospherical electricity, were the continual appearance of the *cirrus* cloud, which, like Proteus, was for ever changing its shape, and presenting itself to the eye under new figures; the prevalence of strong easterly and variable winds; and dry air. Among other circumstances I remarked the appearance of numerous small meteors, or falling stars as they are commonly called, during the night.

Similar appearances again recurring.

Shooting stars have different appearances.

The same kind of weather has returned again this autumn, marked by similar circumstances, and the small meteors have again been numerous. On this last circumstance I dwell particularly; for I have observed, that these meteors vary very considerably in appearance according to the kind of weather which prevails. Those which I have alluded to, and which are usually seen during the prevalence of clear *dry* weather and easterly winds, are small, they shoot along very rapidly, and leave little or no train behind them; they have so much the colour and general appearance of the stars, that they have hence received their vulgar appellation. Similar to these are those which are common in clear frosty winter nights. Larger ones than these generally attend warm summer evenings, particularly when *cirro-cumulus* and thunder clouds abound*, with easterly winds. On the 10th of last month, a showery day with northerly wind was followed by a very clear night abounding with small meteors, but they were of a very peculiar and unusual kind, being of a blueish white colour, like the burning of phosphorus, and they left long trains behind them, of the same colour, which lasted for two or three seconds after their extinction. I suppose in the space of an hour I saw above thirty of them, but they were all of this kind, and left the long white tails, which remained for some seconds in the tract in which the stars had gone.

Some of a peculiar appearance noticed.

These kind of meteors are strikingly different from the common kind noticed above; I have sometimes seen them before, but it has always been in the clear intervals of *showery* weather, previous to the occurrence of *high wind*: it was probably this sort of meteor to which Virgil alluded as a prognostic of windy weather.

These have been seen only in the clear intervals of showery weather, followed by high winds.

Alluded to by Virgil.

> Sæpè etiam stellas, vento impendente, videbis
> Præcipites cœlo labi, noctisque per umbram
> Flammarum longos a tergo albescere tractus.
>
> *Georg lib. i, v.* 360.

On the evening of the 25th of last June I saw a meteor, which was a perfectly stationary accension, and lasted scarcely a second; it was followed by many days of damp rainy weather.

A stationary meteor.

I wish that Meteorologists would note down the peculiarities observable in meteors in their monthly journals.

I shall conclude by observing, that, if these considerations should appear trifling and frivolous to any of your readers, it must be remembered, that it is only by accurate and repeated observation of a multitude of phenomena, that the science of meteorology can be brought to any degree of perfection.

I remain, Sir, yours &c.

Clapton, Sept. the 18*th*,
1811.

THOMAS FORSTER.

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* I do not allude to those very large meteors, which occasionally appear: Such for example, as that seen in August, 1783.

XI.

XI.

On the Composition of Zeolite. *By* JAMES SMITHSON, *Esq. F. R. S*[*].

Species of minerals can be ascertained only by chemistry.

MINERAL bodies being, in fact, *native chemical preparations*, perfectly analogous to those of the laboratory of art, it is only by chemical means, that their species can be ascertained with any degree of certainty, especially under all the variations of mechanical state and intimate admixture with each other, to which they are subject.

And accordingly, we see those methods, which profess to supersede the necessity of chemistry in mineralogy, and to decide upon the species of it by other means than hers, yet bring an unavoidable tribute of homage to her superior powers, by turning to her for a solution of the difficulties, which continually arise to them; and to obtain firm grounds to relinquish or adopt the conclusions, to which the principles they employ lead them.

Zeolite and natrolite supposed to be distinct species.

Zeolite and natrolite have been universally admitted to be species distinct from each other, from Mr. Klaproth having discovered a considerable quantity of soda and no lime, in the composition of the latter, while Mr. Vauquelin had not found any portion of either of the fixed alkalis, but a considerable one of lime, in his analysis of zeolite[†].

The natrolite has been lately met with under a regular crystalline form, and this form appears to be perfectly similar to that of zeolite; but Mr. Haüy has not judged himself warranted by this circumstance, to consider these two bodies as of the same species, because zeolite, he says, "does not contain an atom of soda[‡]."

Soda found in supposed zeolites long ago;

I had many years ago found soda in what I considered to be zeolites, which I had collected in the island of Staffa, having formed Glauber's salt by treating them with sulphuric acid; and I have since repeatedly ascertained the presence of the same principle in similar stones from various other places; and Dr. Hutton and Dr. Kennedy had likewise detected soda in bodies, to which they gave the name of zeolite.

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[*] Phil. Trans. for 1811, p. 171. [†] Journal des Mines, No. XLIV.
[‡] Journal des Mines, No. CL, Juin 1809, p. 458.

but their identity with Haüy's mesotype not ascertained.

There was, however, no certainty, that the subjects of any of these experiments were of the same nature as what Mr. Vauquelin had examined, were of that species which Mr. Haüy calls mesotype.

A specimen sent by Mr. Haüy.

Mr. Haüy was so obliging as to send me lately some specimens of minerals. There happened to be among them a cluster of zeolite in rectangular tetrahedral prisms, terminated by obtuse tetrahedral pyramids, the faces of which coincided with those of the prism. These crystals were of a considerable size, and perfectly homogeneous, and labelled by himself "*Mesotype pyramidée du depart. du Puy de Dôme.*" I availed myself of this very favourable opportunity, to ascertain whether the mesotype of Mr. Haüy and natrolite did or did not differ in their composition, and the results of the experiments have been entirely unfavourable to their separation, as the following account of them will show.

This zeolite, or mesotype, analysed.

10 grains of this zeolite being kept red hot for five minutes lost 0·75 of a grain, and became opaque and friable. In a second experiment, 10 grains, being exposed for 10 minutes to a stronger fire, lost 0·95 of a grain, and consolidated into a hard transparent state.

10 grains of this zeolite, which had not been heated, were reduced to a fine powder, and diluted muriatic acid poured upon it. On standing some hours, without any application of heat, the zeolite entirely dissolved, and some hours after, the solution became a jelly: this jelly was evaporated to a dry state, and then made red hot.

Water was repeatedly poured on this ignited matter, till nothing more could be extracted from it. This solution was gently evaporated to a dry state, and this residuum made slightly red hot. It then weighed 3·15 grains. It was *muriate of soda*.

The solution of this muriate of soda, being tried with solutions of carbonate of ammonia and oxalic acid, did not afford the least precipitate, which would have happened had the zeolite contained any lime, as the muriate of lime[*] would not have been decomposed by the ignition.

The remaining matter, from which this muriate of soda had been extracted, was repeatedly digested with marine acid, till all that was soluble was dissolved. What remained was silica, and, after being made red hot, weighed 4·9 grains.

The muriatic solution, which had been decanted off from the silica, was exhaled to a dry state, and the matter left made red hot. It was alumina.

To discover whether any magnesia was contained among this alumina, it was dissolved in sulphuric acid, the solution evaporated to a dry state, and ignited. Water did extract some saline matter from this ignited alumina, but it had not at all the appearance of sulphate of magnesia, and proved to be some sulphate of alumina, which had escaped decomposition, for on an addition of sulphate of ammonia to it, it produced crystals of compound sulphate of alumina and ammonia, in regular octahedrons.

This alum and alumina were again mixed and digested in ammonia, and the whole dried and made red hot. The alumina left weighed 3·1 grains.

Being suspected to contain still some sulphuric acid, this alumina was dissolved in nitric acid, and an excess of acetate of barytes added. A precipitate of sulphate of barytes fell, which after being edulcorated and made red hot, weighed 1·2 grains. If we admit $\frac{1}{3}$ of sulphate of barytes to be sulphuric acid, the quantity of the alumina will be $= 3·1 — 0·4 = 2·7$ grains.

From the experiments of Dr. Marcet[†], it appears, that 3·15 grains of muriate of soda afford 1·7 grain of soda.

Hence, according to the foregoing experiments, the 10 grains of zeolite analysed consisted of

Its component parts.

Silica	4·90
Alumina	2·70
Soda	1·70
Ice	0·95
	10·25

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[*] These names are retained for the present, as being familiar, though since Mr. Davy's important discovery of the nature of what was called oximuriatic acid, the substances, to which they are applied, are known not to be salts, but metallic compounds analogous to oxides.

[†] Phil. Trans. 1807 : or Journal, vol. XX, p. 30.

As these experiments had been undertaken more for the purpose of ascertaining the nature of the component parts of this zeolite than their proportions, the object of them was considered as accomplished, although perfect accuracy in the latter respect had not been attained, and which, indeed, the analysis we possess of natrolite by the illustrious chemist of Berlin renders unnecessary.

Reasons for retaining the name of zeolite.

I am induced to prefer the name of zeolite for this species of stone, to any other name, from an unwillingness to obliterate entirely from the nomenclature of mineralogy, while arbitrary names are retained in it, all trace of one of the discoveries of the greatest mineralogist who has yet appeared; and which, at the time it was made, was considered as, and was, a very considerable one, being the first addition of an earthy species, made by scientific means, to those established immemorially by miners and lapidaries, and hence having, with tungsten and nickel, led the way to the great and brilliant extension, which mineralogy has since received. And, of the several substances, which, from the state of science in his time, certain common qualities induced Baron Cronstedt to associate together under the name of zeolite; it is this which has been most immediately understood as such, and the qualities of which have been assumed as the characteristic ones of the species.

Names given by discoverers should not be altered.

Indeed, I think, that the name imposed on a substance by the discoverer of it ought to be held in some degree sacred, and not altered without the most urgent necessity for doing it. It is but a feeble and just retribution of respect for the service, which he has rendered to science.

Existence of phosphoric acid suspected in it, but none found.

Professor Struve, of Lausanne, whose skill in mineralogy is well known, having mentioned to me, in one of his letters, that, from some experiments of his own, he was led to suspect the existence of phosphoric acid in several stones, and particularly in the zeolite of Auvergne, I have directed my inquiries to this point, but have not found the phosphoric, or any other acknowledged mineral acid, in this zeolite.

Is quartz an acid?

Many persons, from experiencing much difficulty in comprehending the combination together of the earths, have been led to suppose the existence of undiscovered acids in stony crystals. If quartz be itself considered as an acid, to which order of bodies its qualities much more nearly assimilate it, than to the earths, their composition becomes readily intelligible. They will then be neutral salts, silicates, either simple or compound. Zeolite will be a compound salt, a hydrated silicate of alumina and soda, and hence a compound of alumina not very dissimilar to alum. And topaz, the singular ingredients of which, discovered by Mr. Klaproth, have called forth a query from the celebrated Mr. Vauquelin, with regard to the mode of their existence together[*], will be likewise a compound salt, consisting of silicate of alumina, and fluate of alumina.

Is zeolite a hydrated tourmaline?

Our acquaintance with the composition of the several mineral substances, is yet far too inaccurate, to render it possible to point out with any degree of certainty the one of which zeolite is a hydrate, however the agreement of the two substances in the nature of their constituent parts, and in their being both electrical by heat, directs conjecture towards tourmaline.

St. James's Place, Jan. 22, 1811.

Addition to the Account of native Minium.

After I had communicated to the President the account of the discovery of native minium, printed in the Philosophical Transactions for 1806[†], I learned, that this ore came from the lead mines of Breylau in Westphalia.

XII.

Extract from a Paper communicated to the American Philosophical Society on the Discovery of Palladium in a Native Alloy of Gold; by Mr. J. Cloud, *Director of the Chemical Processes at the Mint of the United States*[‡].

Gold from Portuguese America.

In 1807 about 820 ounces of gold bullion were brought into the mint of the United States. They consisted of 120 small ingots, each stamped on one side with the arms of Portugal, and the inscription *Rio das montis*, and on the other with a globe. The fineness of each ingot too was marked on it. Among these were two differing from the others so much in colour, that Mr. Cloud preserved one, weighing 3 oz, 11 dwts, 12 grs, to examine it. The following were the experiments he made.

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[*] Annales du Museum d'Hist. Nat. tome 6, p. 24.

[†] See Journal, vol. XVI, p. 127.

[‡] Annal. de Chim. vol LXXIV, p. 99.

Analysed.

No silver.

1. Nitromuriatic acid was employed on one portion of the ingot, to find whether it contained any silver; and none was discovered.

No metal easily oxidable.

2. Twenty four carats were mixed with 48 carats of fine silver, and cupelled with lead, to separate any oxidable metal that might be present: but there was no diminution of weight, consequently the alloy contained no metal easily oxidable.

Gold.

3. The fine metals of the preceding experiment were flattened between rollers, and subjected to the action of pure nitric acid. The silver and the native alloy mixed with the gold, were dissolved by the acid, which acquired a deep brown red colour. The metal that remained, washed with pure water, and dried by the fire, weighed 22 carats 1½ gr. It had all the appearance of fine gold.

No platina.

4. The metals not dissolved in the latter experiment were subjected to the action of nitromuriatic acid. The whole was dissolved, except a small quantity of silver, which had escaped the action of the nitric acid. The solution was assayed with muriate of ammonia, and other tests, from an expectation of finding platina, but no trace of this metal was discovered. The gold thrown down was pure to [illegible]

Some other metal

5. Pure muriatic acid was poured into the metallic solution resulting from Exp. 3, till the silver was completely thrown down, and the acid was in considerable excess. None of the colouring matter was precipitated from the solution, which remained red, and did not appear at all changed, notwithstanding the precipitation of the silver.

Palladium.

From these preliminary experiments it appeared, that the alloy was a compound of gold and some metal capable of resisting cupellation, and soluble both in nitric and nitromuriatic acids. In adopting the following mode of analysis evident proofs of the existence of a metal possessing all the properties of palladium were obtained.

Analysis of the whole.

I. The whole ingot was combined with twice its weight of fine silver, and cupelled with lead equal in weight to the mixture.

II. The cupelled metals were reduced to thin plates, and kept in boiling nitric acid till the silver and palladium were dissolved. The deep brown red solution was decanted, and the remaining gold washed with distilled water, which was afterward mixed with the decanted solution.

III. Pure muriatic acid was added to the preceding solution, till it was in excess, and nothing more fell down. The liquid retaining its red colour was decanted off, and the precipitate washed with distilled water. The waters of elutriation were added to the decanted liquor, which then held nothing in solution but palladium.

IV. A solution of pure potash * was poured into the metallic solution of the preceding experiment, till the whole of the palladium was thrown down in a brown flocculent precipitate. This was washed with distilled water, collected on a filter, and dried.

V. A portion of the precipitate obtained in this experiment was put into a crucible without addition, and exposed to a heat of about 60° of Wedgwood; when a metallic button of palladium was obtained of the spec. grav. of 11 041.

VI. Another portion of the precipitate of Exp. IV was mixed with black flux, and exposed to the same degree of heat as in the preceding experiment. The result was the same.

The alloy was palladium;

A metal supposed to be palladium, thus obtained from a source where it was not known to exist, required to be compared with the palladium obtained from crude platina, to confirm its identity. Comparative experiments were accordingly made with prussiate of mercury, fresh muriate of tin, and other tests. The metals from these two sources did not exhibit the least difference.

and no other was present.

Native gold is never found perfectly pure. Hitherto it

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* Carbonate of potash will not answer so well, because part of the palladium would remain dissolved in the carbonic acid.

has always been seen alloyed with silver or copper, and most commonly with both, and with other metals also. The gold that was the subject of the preceding experiments appears to have been alloyed with palladium alone. If it had been alloyed with any other metal, except silver or platina, the experiment No. II would have shown it; and silver would have been discovered by the first experiment, and platina by the fourth.

XIII.

Analysis of the Cement of an antique Mosaic, found at Rome: by Mr. D'ARCET *.

The cement described.

THIS cement is of a yellowish white, very compact, without grains, and pretty hard. It blackens a little in the fire. Before calcination it effervesces briskly: but after it has been calcined, nitrous acid dissolves it without evolving any carbonic acid. In the former case a few yellowish flocks remain, and some fragments of a reddish brown colour, but little compact, and resembling the porous lavas, or puzzolana. The yellowish flocks are destructible in the fire.

Sulphuric acid precipitates nothing from these solutions, therefore they contain no lead.

Ammoniac throws down only a little alumine and oxide of iron.

Analysis of it

5 gram. [77·29 grs] of this cement, calcined under a muffle for eight hours, no longer effervesced, and weighed only 2·815 gr. [43·48 grs]; which indicates in 100 parts

56·3 of quicklime; and

43·7 of vegetable or animal matter and carbonic acid.

10 grammes of this cement left 4·1 of carbonic acid, when acted on by nitric acid. We have therefore in 100 parts

59 of quicklime and animal or vegetable matter, and

41 of carbonic acid.

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* Ann. de Chim. vol LXXIV, p. 313. This cement was sent by Mr. Belloni, Director of the Imperial School of Mosaic, who considered it as one of the best cements the ancients had employed in the fabrication of their mosaics, and of their pavements in compartments.

On comparing these two analyses we find, that the cement contains in 100 parts

Quicklime	56·3
Carbonic acid	41
Vegetable or animal matter	2·7
	100·0

The lime nearly saturated.

In this cement, we see, the lime, if it were employed quick, has resumed from the air, and in the lapse of time, nearly all the carbonic acid necessary for its saturation.

This singular.

How effected.

This is the first time of my observing this fact. As I have never found the lime in mortar, however ancient, saturated with carbonic acid, I am inclined to suppose, that the vegetable or animal matter, that served as a gluten, promoted the absorption of carbonic acid; or rather, that the cement in question was made with carbonate of lime (whiting), and not with quicklime.

In the latter case about 97 parts of carbonate of lime, and 3 of oil, glue, or cheese, must have been employed.

In the former the cement would have been composed of about 56 parts of quicklime to 3 of vegetable or animal matter.

It is obvious, that these proportions, which are found at present to form the cement in question, were not followed in its preparation.

If oil were employed, it would have increased in weight in drying; and then less than 0·03 must have been used, which appears to me impossible.

Its probable composition.

It is more than probable therefore, that the substance employed was analogous to the caseous part of milk, and then it would have diminished in weight by losing the water it contained, which served to reduce to a paste the lime or carbonate of lime.

From this analysis it appears, that the cement was very simple; and that those we now compose on the same principle would become equally hard in time.

XIV.

XIV.

METEOROLOGICAL JOURNAL.

	Wind	PRESSURE. Max.	Min	Med.	TEMPERATURE. Max	Min	Med.	Evap	Rain	
8th Mo.										
AUG. 11	N W	30·16	29·86	30·01	61	42	51·5	—		☽
12	N W	30·16	30·10	30·13	64	50	57	—		
13	S W	30·24	30·10	30·17	73	52	62·5	·33	—	
14	N W	30·25	30·09	30·17	66	47	56·5	—		
15	S W	30·25	29·97	30·11	68	51	59·5	—	—	
16	S W	30·14	29·97	30·055	68	57	62·5	·37	—	
17	N W	30·13	30·03	30·08	70	45	57·5	—		
18	E	30·03	29·76	29·895	72	55	63·5	—	2	
19	Var.	29·72	29·65	29·685	68	54	61	·30	·35	●
20	W	30·05	29·72	29·885	64	57	60·5	—	5	
21	S W	30·08	30·04	30·06	68	56	62	—		
22	W	30·04	29·92	29·98	71	52	61·5	·41	—	
23	S	29·92	29·73	29·825	68	55	61·5	—	4	
24	E	29·73	29·52	29·625	70	55	62·5	—	—	
25	S W	29·70	29·50	29·60	65	48	56·5	·16	·39	
26	S W	29·78	29·70	29·74	67	56	63·5	—		
27	W	30·07	29·74	29·905	68	44	56	—	1	☾
28	Var.	30·11	30·03	30·07	66	51	58·5	·42		
29	S W	30·17	29·96	30·065	69	46	57·5	—	2	
30	N W	30·20	30·13	30·165	69	47	58	·24		
31	S W	30·02	29·97	29·995	71	53	62	—		
9th Mo.										
SEPT. 1	N W	30·22	30·02	30·12	68	45	56·5	—		
2	N	30·29	30·26	30·275	65	45	55	—		○
3	N E	30·29	30·24	30·265	64	53	58·5	·34		
4	N E	30·24	30·18	30·21	62	53	57·5	—		
5	E	30·18	30·15	30·165	71	52	61·5	—		
6	E	30.17	30·13	30·15	73	44	58·5	·35		
7	N E	30·19	30·13	30·16	72	43	57·5	—		
8	E	30·20	30·17	30.185	74	47	60·5	·22		
		30·29	29·50	30·025	74	42	59·20	3·14	·88	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES

NOTES.

Eighth Mo. 11. *Cumulostratus*, dense about noon, but which soon after dispersing, a brilliant sunset ensued 12. a. m. cloudy: wind S.W. 13. A few drops at intervals: rain in the S. by inosculation. 14. a. m. *Cumulus*, with haze gradually increasing above: p. m. clouds below disperse: a fine elevated veil of *cirrus*, coloured at sunset. 15. Elevated clouds, with traces of *cumulus*: some large drops about noon: at sunset, the western sky richly coloured with red and yellow, on *cirrocumulus* passing to *cirrostratus*: windy night. 16. a. m. Windy: p. m. small rain: clear evening, with coloured *cirrus* and *cirrocumulus*. 18. Evening, large *cirri*, pointing upwards. 19. a. m. Thunder showers, chiefly to S. S. W. and N. A strong variable charge in the insulated conductor. 6 p. m. Fair and windy, with *cumulostratus*. 20, 21. Windy: much dew. 22. Light rain a. m.: showers p. m. 23. Misty morning: *cumulus*, with *cirrostratus* from the S.: about *one*, these inosculated, and showers prevailed, p m. 24. Misty morning: *cumulostratus*: a few drops of rain evening, *cirrostratus*. 25. Misty, and raining at 8½ a. m. Wind S. E. Evening, *cumulostratus* evaporating, beneath a veil of *cirrus*, which at the moment of sunset, was of a light silver grey, and during twilight, passed through yellow, orange, red, and purple, to dull grey; and lastly became again somewhat red much dew, with a very moist air. 26. A small lunar halo, on clouds moving in a northerly current 27. Windy, a. m. small rain, evening much dew. 28. Windy. 30. a. m. *Cirrus*, with points dependent and crossing, and *cumulus* forming beneath: at 9 p. m. *Cirrocumulus*, with much dew. The barometer unsteady. 31. Fine day. *cumulus, cirrus, cirrocumulus*. a diffused blush on the twilight, which begins to be very luminous.

RESULTS.

Wind westerly, with little exception, to the time of full moon, when it came round by N. to the Eastward.

Barometer: highest observation 30·29 in. lowest 29·50 in.
Mean of the period 30·025 in

Therm.: highest observation 74°, lowest 42°. Mean of the period 59·20°.
Evaporation 3·14 in. Rain 0·88 in.

L. HOWARD.

PLAISTOW,
Ninth Month 26, 1811.

XV.

XV.

Remarks on the Inclination of the Stems of Plants toward the Light: *by M.* DECANDOLLE *.

Inclination of stems of plants toward the light, not from volition or instinct, but known laws of vegetation.

OF all the phenomena that living vegetables exhibit, there are few appear so extraordinary, as the energy and constancy with which their stems incline toward the light. Not only has no explanation been given hitherto of this fact by any physiologist, but writers have even been found, who, more of the poet than of the naturalist, have ascribed this tendency to some kind of instinct or volition in plants. I think I can prove in a few words, that it is a simple and necessary consequence of the known laws of vegetation. What I have to say in this respect will even appear of so elementary a nature, that every one will be surprised not to have met with in all books: and that I shall be pardoned for writing it only on account of the wanderings, into which some have gone on the subject.

Etiolation, not a general but topical affection.

Every one knows, that the state of silvery whiteness and extraordinary elongation, acquired by plants that grow in darkness, is designated by the term etiolation. All who have studied this disease know, that it is not a general disease, but a local affection; as I have satisfied myself by direct experiments. If we expose to the light of day an etiolated plant, in two days it will acquire a green colour perceptibly similar to that of plants, which have grown in open daylight. If we expose to the light one part of the plant, be it leaf or branch, this part alone will become green. If we cover any part of a leaf with an opake substance, this place will remain white, while the rest becomes green. The whiteness of the inner leaves of cabbages is a partial etiolation, and a thousand other examples might easily be quoted. Etiolation therefore is certainly a local, and not a general disease.

Various degrees of it.

On the other hand it is equally certain, that between complete etiolation and complete verdure every possible intermediate degree exists, determined by the intensity of the light. Of this any one may easily satisfy himself, by attending to the colour of a plant exposed to the full daylight; it exhibits in succession all the degrees of verdure. I had already seen the same phenomenon in a particular manner, by exposing etiolated plants to the light of lamps. In these experiments (inserted in vol. I, of the *Mém. des Savans étrangers*) I not only saw the colour come on gradually according to the continuance of the exposure to light; but I satisfied myself, that a certain intensity of permanent light never gives to a plant more than a certain degree of colour. The same fact readily shows itself in nature, when we examine the plants that grow under shelter or in forests, or when we examine in succession the state of the leaves, that form the heads of cabbages.

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* Mém, de la Soc. d'Arcueil, vol. II, p. 104.

Etiolated plants exposed to artificial light.

Now let us examine the state of a plant, that is not equally enlightened on all sides, as we see them in forests; and still better in plants cultivated in hothouses, or in common rooms. That part of the stalk which is exposed to the least light must necessarily be a little more etiolated than the other; consequently it must elongate itself a little more, while the fibres on the side next the light must become on the contrary a little more short and stiff. But it is evident, that this inequality of elongation between the fibres of the two opposite sides cannot take place without the extremity of the stalk tending to incline toward the side where the fibres are shortest, that is to say, on the side next the light.

Plants not equally exposed to light in forests, hot houses, or rooms, incline to the light from partial etiolation.

Thus it appears, if this theory be true, that the energy, with which plants incline themselves toward the light, must be proportional to the inequality of the light they receive on opposite sides, and to the greater or less propensity to etiolation, that each plant, or part of a plant, possesses, in consequence of its structure. This I shall proceed to prove by facts, most of them, it is true, already known, but which will be so many confirmations of my hypothesis.

This inclination proportional to the degree of the affection.

The parts of plants liable to etiolation alone possess this tendency to incline toward the light. Of this any one may satisfy himself, by examining the branches directed toward the windows in a greenhouse not well lighted. He will find, that they are always the young shoots, capable of emitting oxigen gas, that direct themselves toward the light; and

Only parts of plants liable to etiolation incline toward the light.

that the energy of this direction is greatest in the most herbaceous stalks, in which the phenomenon of etiolation is also most remarkable. In forests the woody branches or stems themselves may frequently be observed twisted to gain an open place; but this is because the unequal distribution of light has continued several years; the branches were bent in their green state, and have acquired solidity in that in which they are found. Of this I have satisfied myself by direct measures. Permit me here to observe, that it may be possible to avail ourselves of this property of vegetables, to formed curved timber for the purposes of the arts, by directing the light on certain trees in a suitable manner.

The inclination in general proportionate to the flexibility,

but does not take place in plants incapable of decomposing carbonic acid, as dodder.

In the instances I have quoted it may be supposed, that, if old branches do not bend, it is solely on account of their hardness: and indeed it is evident, that, the more flexible the branch, the more will it be bent by the same quantity of partial etiolation; but a striking example will prove, that the inclination toward the light does not take place in the most flexible branches, when they want the faculty of decomposing carbonic acid gas by means of light. This example is dodder. I have satisfied myself by direct experiments, that it does not incline itself toward the light; that, placed under water in the sun, it does not decompose carbonic acid gas, and consequently can lengthen itself equally on both sides, though unequally illumined.

It depends on a partial elongation of the vessels,

and is scarcely perceptible in plants formed of spherical cells.

The whole of the phenomenon then consists in the partial elongation produced by etiolation. But it is known, that the elongation takes place chiefly in the vessels, which draw along with them as it were the cellular texture. Consequently, the more vessels there are in a plant, or a part of a plant, the more it ought to incline toward the light. In plants totally destitute of vessels, this inclination must be scarcely perceptible, because the rounded cells grow nearly alike in all directions: hence this inclination toward the light is next to nothing in the cryptogamia, as in certain algæ composed solely of rounded cellular texture. Those of the cryptogamia, which, as the mosses for example, are composed of two sorts of cellular texture, one with rounded the other with tubular cells, approach the vascular plants in consequence of the latter, which is capable of more or less elongation; and in these we may observe a slow and feeble inclination toward the light. Lastly, plants furnished with vessels, and of these plants the stems, in which vessels most abound, exhibit this inclination most forcibly.

I conceive therefore I have proved, by this combination of facts, that the hitherto unexplained phenomenon of the inclination of the stems of plants toward the light is readily reducible to the known laws of etiolation.

XVI.

On the Forcing-houses of the Romans, with a List of Fruits cultivated by them, now in our Gardens. By the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. P. R. S. &c.* *

The Romans had forcing-houses.

MR. A. Knight was the first person among us members of the Horticultural Society, who observed, in reading Martial, strong traces of the Romans having enjoyed the luxury of forcing-houses. I shall cite the principal passages upon which he has founded this observation, the truth of which is not likely to be controverted, and add such remarks as present themselves upon the Roman hot-houses, with a few words on the subject of our own.

The first epigram is as follows:

Proofs of this.

Pallida ne Cilicum timeant pomaria brumam,
  Mordeat et tenerum fortior aura nemus,
Hibernis objecta notis specularia puros
  Admittunt soles, et sine fæce diem, &c.
Martial, lib. viii, 14.

Qui Corcyræi vidit pomaria regis,
  Rus, Entelle, tuæ præferat ille domus.
Invida purpureos urat ne bruma racemos,
  Et gelidum Bacchi munera frigus edat;
Condita perspicua vivit vindemia gemma,
  Et tegitur felix, nec tamen uva latet.

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* Trans of the Hort Soc. vol. I, p. 147.

> Fœmineum lucet sic per bombycina corpus:
> Calculus in nitida sic numeratur aqua.
> Quid non ingenio voluit natura licere?
> Autumnum sterilis ferre jubetur hiems.
> *Martial*, lib. viii, 68.

The four last lines of the first epigram are omitted, as having no reference whatever to the subject.

Their mode of forcing cucumbers.

From these passages, and from that of Pliny, in which he tells us that Tiberius, who was fond of cucumbers, had them in his garden throughout the year by means of (*specularia*) stoves, where they were grown in boxes, wheeled out in fine weather, and replaced in the nights or in cold weather, *Pliny, book* xix, *sect.* 23, we may safely infer, that forcing-houses were not unknown to the Romans, though they do not appear to have been carried into general use.

Flues in common use among them.

Flues the Romans were well acquainted with; they did not use open fires in their apartments as we do, but, in the colder countries at least, they always had flues under the floors of their apartments. Mr. Lysons found the flues, and the fire-place whence they received heat, in the Roman villa he has described in Gloucestershire; in the baths also, which no good house could be without, flues were used to communicate a large proportion of heat for their sudatories, or sweating apartments.

They used talc instead of glass.

The article with which their windows were glazed, if the term may be used, was *talc*, or what we call Muscovy glass, (*lapis specularis*). At Rome, the apartments of the bettermost classes were furnished with curtains (*vela* *), to keep away the sun; and windows (*specularia* †), to resist cold; so common was the use of this material for windows, that the glazier, or person who fitted the panes, had a name, and was called *specularius.*

The first epigram relates to a peach-house

On the epigrams the following remarks present themselves. The first in all probability described a peach-house, the word pale, which is meant as a ridicule upon the prac-

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Transparent bee-hives.

* Ulpian l. Quæsitum 12. The Romans also made transparent bee-hives of the same material. Pliny, lib. xxi, sect. 47.

† Quamvis coenationem velis et specularibus muniant. *Seneca.*

tice, gives reason for this supposition: we all know that peaches grown under glass cannot be endowed either with colour or with flavour, unless they are exposed by the removal of the lights, from the time of their taking their sesond swell, after stoning, to the direct rays of the sun: if this is not done, the best sorts are pale green when ripe, and not better than turnips in point of flavour; but it is not likely, that a Roman hot-house should, in the infancy of the invention, be furnished with movable lights, as ours are. The Romans had peaches in plenty both hard and melting*. The flesh of the hard peaches adhered to the stones as ours do *, and were preferred in point of flavour to the soft ones †.

They had both sorts in plenty.

The second epigram refers most plainly to a grape-house, but it does not seem to have been calculated to force the crop at an earlier period than the natural one; it is more likely to have been contrived for the purpose of securing a late crop, which may have been managed by destroying the first set of bloom, and encouraging the vines to produce a second. The last line of the epigram, which states the office of the house to be that of compelling the winter to produce autumnal fruits, leads much to this opinion.

The second epigram describes a grape house for late crops.

Hot-houses seem to have been little used in England, if at all, in the beginning of the last century. Lady Mary Wortley Montagu, on her journey to Constantinople, in the year 1716, remarks the circumstance of pine-apples being served up in the desert, at the Electoral table at Hanover, as a thing she had never before seen or heard of; *see her Letters*. Had pines been then grown in England, her ladyship, who moved in the highest circles, could not have been ignorant of the fact. The public have still much to learn on the subject of hot-houses, of course the Horticultural Society have much to teach,

Hot-houses scarcely known a century in England.

They have hitherto been too frequently misapplied under the name of forcing-houses, to the vain and ostentatious purpose of hurrying fruits to maturity, at a season of the year, when the sun has not the power of endowing them with their natural flavour; we have begun however to apply

Misapplied as forcing-houses.

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* Pliny, lib. xv, sect. 34.

† Pliny, lib. xv, sect. 11.

Their proper uses.

them to their proper use, we have peach-houses built for the purpose of presenting that excellent fruit to the sun, when his genial influence is the most active. We have others for the purpose of ripening grapes, in which they are secured from the chilling effects of our uncertain autumns, and we have brought them to as high a degree of perfection here, as either Spain, France, or Italy can boast of. We have pine-houses also, in which that delicate fruit is raised in a better style than is generally practised in its native intertropical countries; except, perhaps, in the well managed gardens of rich individuals, who may, if due care and attention is used by their gardeners, have pines as good, but cannot have them better, than those we know how to grow in England.

They will be much improved.

The next generation will no doubt erect hot-houses of much larger dimensions than those, to which we have hitherto confined ourselves, such as are capable of raising trees of considerable size; they will also, instead of heating them with flues, such as we use, and which waste in the walls that conceal them more than half of the warmth they receive from the fires that heat them, use naked tubes of metal filled with steam[*] instead of smoke. Gardeners will then be enabled to admit a proper proportion of air to the trees in the season of flowering, and as we already are aware of the use of bees in our cherry-houses to distribute the pollen, where wind cannot be admitted to disperse it, and of shaking the trees when in full bloom, to put the pollen in motion, they will find no difficulty in setting the shyest kinds of fruits.

Fruits that will soon be cultivated in them.

It does not require the gift of prophecy to foretell, that ere long the aki and the avocado pear of the West Indies, the flat peach, the mandarine orange, and the litchi of China, the mango[†], the mangostan, and the durion of the East Indies, and possibly other valuable fruits, will be frequent at the tables of opulent persons; and some of them, perhaps in less than half a century, be offered for sale on every market day at Covent Garden.

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[*] A neat and ingenious fancy for heating melon frames by steam appeared in the Gentleman's Magazine for January, 1755

[†] The mango was ripened by Mr. Aiton, his Majesty's gardener, in the Royal Gardens at Kew, in the autumn of 1808, who has frequently ripened fruits of the mespilus japonica, which is a good but not a superior fruit.

Subjoined is a list of those fruits cultivated at Rome, in the time of Pliny, that are now grown in our English gardens.

Modern fruits cultivated by the Romans.

Almonds.—Both sweet and bitter were abundant.

Apples.—22 sorts at least: sweet apples (*melimala*) for eating, and others for cookery. They had one sort without kernels.

Apricots.—Pliny says of the apricot (*armeniaca*) quæ sola et odore commendantur, *lib.* xv, *sect.* 11. He arranges them among his plums. Martial valued them little, as appears by his epigram, xiii, 46.

Cherries were introduced into Rome in the year of the city 680, 73 A. C. and were carried thence to Britain 120 years after, A. D. 480. The Romans had eight kinds, a red one, a black one, a kind so tender as scarce to bear any carriage, a hard fleshed one (*duracina*) like our bigarreau, a small one with a bitterish flavour (*laurea*) like our little wild black, also a dwarf one not exceeding three feet high.

Chestnuts.—They had six sorts, some more easily separated from the skin than others, and one with a red skin; they roasted them as we do.

Figs.—They had many sorts, black and white, large and small, one as large as a pear, another no larger than an olive.

Medlars.—They had two kinds, the one larger, and the other smaller.

Mulberries.—They had two kinds of the black sort, a larger and a smaller. Pliny speaks also of a mulberry growing on a brier: *Nascuntur et in rubis*, *l.* xv, *sect.* 27, but whether this means the raspberry, or the common blackberry does not appear.

Nuts—They had hazle-nuts and filberds; (has quoque mollis protegit barba) *l.* 15, *sect.* 24: they roasted these nuts.

Pears.—Of these they had many sorts, both summer and winter fruit, melting and hard, they had more than thirty-six kinds, some were called libralia: we have our pound pear.

Plums.—They had a multiplicity of sorts, (ingens turba prunorum) black, white, and variegated, one sort was called asinina, from its cheapness, another damascena, this had much stone and little flesh: from *Martial's Epigram*, xiii, 29, we may conclude, that it was what we now call prunes.

Quinces.—They had three sorts, one was called chrysomela from its yellow flesh; they boiled them with honey, as we make marmalade. See *Martial*, xiii. 24.

Services.—They had the apple shaped, the pear-shaped, and a small kind, probably the same as we gather wild, possibly the azarole.

Strawberries—they had, but do not appear to have prized, the climate is too warm to produce this fruit in perfection unless in the hills.

Vines.—They had a multiplicity of these, both thick skinned (duracina) and thin skinned: one vine growing at Rome produced 12 amphoræ of juice, 84 gallons. They had round berried, and long berried sorts, one so long, that it was called dactylides, the grapes being like the fingers on the hand. Martial speaks favourably of the hard skinned grape for eating, xiii, 22.

Walnuts.—They had soft shelled, and hard shelled, as we have: in the golden age, when men lived upon acorns, the gods lived upon walnuts, hence the name juglans, Jovis glans.

Fruit cultivated in England in the 16th century.

As a matter of curiosity, it has also been deemed expedient, to add a list of the fruits cultivated in our English gardens, in the year 1573: it is taken from a book entitled Five Hundred Points of good Husbandry, &c., by Thomas Tusser.

Thomas Tusser.

Thomas Tusser, who had received a liberal education at Eton school, and at Trinity Hall, Cambridge, lived many years as a farmer in Suffolk and Norfolk: he afterward removed to London, where he published the first edition of his work under the title of One Hundred Points of good Husbandry, in 1557.

In his fourth edition, from which this list is taken, he first introduced the subject of gardening, and has given us not only a list of the fruits, but also of all the plants then cultivated in our gardens, either for pleasure or profit, under the following heads.

Objects of gardening at that time.

Seedes and herbes for the kychen, herbes and rootes for sallets and sawce, herbes and rootes to boyle or to butter, strewing herbes of all sorts, herbes, branches, and flowers for windowes and pots, herbs to still in summer, necessarie herbes to grow in the gardens for physick not reherst before.

This list consists of more than 150 species, beside the following fruits.

List of old English fruits.

Apple trees of all sorts
Apricockes
Barberries
Boollesse black and white
Cherries red and black
Chestnuts
*Cornet Plums
Damiseus white and black
Filberds red and white
Goseberries
Grapes white and red
Grene or Grass plums
† Hurtil-berries
Medlers or Merles

Mulberry
‡Peaches white and red
Peeres of all sorts
Peer plums black and yellow
Quince Trees
Raspis
§ Reisons
Small Nuts
Strawberries red and white
Service Trees
Wardens white and red
Wallnuts
Wheat Plums

Though

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* Probably the fruit of cornus mascula, commonly called cornelian cherry.

† *Hurtleberries*, the fruit of *vacinium vitis idea*, though no longer cultivated in our gardens, are still esteemed and served up at the tables of opulent people in the counties that produce them naturally. They are every year brought to London from the rocky country, near Leith Tower in Surry, where they meet with so ready a sale among the middle classes of the people, that the richer classes scarcely know that they are to be bought.

‡ The *yellow fleshed peach* now uncommon in our gardens, but which was frequent 40 years ago, under the name of the orange peach, was called by our ancestors melicoton.

§ By *reisons* it is probable that currants are meant; the imported fruit of that name of which we make puddings and pies was called by our ancestors *raisin de Corance*.

Though the fig is omitted by Tusser, it was certainly introduced into our gardens before he wrote. Cardinal Pole is said to have imported from Italy that tree, which is still growing in the garden of the archbishop's palace, at Lambeth.

XVII.

Method of preparing Ox-Gall in a concentrated state for Painters, and for other Uses: *by* RICHARD CATHERY, *No. 14, Mead's-row, Lambeth**.

SIR,

A method of keeping gall desirable.

IT has been long a desideratum to find out a method of preparing ox-gall for the use of painters, so as to avoid the disagreeable smell, which it contracts by keeping in a liquid state, and at the same time to preserve its useful properties. I have invented a method of doing it with very little expense, which will be a great saving to those who use gall, as it will prevent it from putrifying, or breeding maggots.

Its uses to artists,

One gall prepared in my method will serve an artist a long time, as it will keep a great number of years. It will be a convenient article for use, as a small cup of it may be placed in the same box which contains other colours, where it will be always ready. The qualities of gall are well known to artists in water-colours, particularly to those who colour prints, as many colours will not, without gall, work free on such paper, on account of the oil that is used in the printing-ink.

The artists who make drawings in water-colours also use gall in the water which they mix their colour with, as it clears away that greasiness, which arises from moist hands upon paper, and makes the colour work clear and bright. My preparation is ready for use in a few minutes, all that is necessary being to dissolve about the size of a pea of it in a table-spoonful of water.

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* Trans. of Soc. of Arts, vol. XXVIII, p. 106. Ten guineas were voted to Mr. Cathery for this invention.

It is also of great use to horsekeepers, sailors, and others, to clean woollen clothes from grease, tar, &c.; and will be found advantageous for many other purposes.

and for cleaning cloth.

If it should meet with the approbation of the Society, I have no objection to prepare it for sale.

I am, Sir,
Your obedient Servant,
RICHARD CATHERY,
Botanical Colourer.

Process for preparing Ox-Gall in a concentrated state, by by Mr. Cathery.

Take a gall fresh from the ox, and put it into a basin, let it stand all night to settle, then pour it off from the sediment into a clean earthen mug, and set it in a saucepan of boiling water over the fire, taking care that none of the water gets into the mug. Let it boil till it is quite thick, then take it out and spread it on a plate or dish, and set it before the fire to evaporate; and when as dry as you can get it, put it into small pots, and tie papers over their tops to keep the dust from it, and it will be good for years*.

Method of preparing it.

Certificates were received from Mr. Gabriel Bayfield, No. 9, Park Place, Walworth; and Mr. William Edwards, No. 9, Poplar Row; both botanical colourers; stating, that they have used the ox-gall prepared by Mr. Cathery, and find it to answer better than gall in a liquid state; that this preparation is free from disagreeable smell, and is much cheaper, as one ox-gall thus prepared will last one person for two years, and be as fresh as if just taken from the ox.

Testimonies of its usefulness to artists.

A Certificate was received from Mr. James Stewart, No. 26, St. Martin's Street, Leicester Square, stating, that he lately belonged to his Majesty's ship the Vestal frigate, and that he took out with him, in a voyage to Newfoundland, a large pot of the prepared ox-gall, for the purpose of washing his greasy clothes for two years; that he found it very serviceable, and to keep its virtue as well as the first day.

and at sea

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* Gall will keep some time, if merely boiled so as to separate the albuminous part, agreeably to the directions of Mr. J. Clark, or professor Proust. See Journal, vol. XVII, p. 341. C.

XVIII.

Letter from Mr. VITALIS, *Professor of Chemistry at Rouen, to Mr. Bouillon-Lagrange, on the Amalgam of Mercury and Silver called Arbor Dianæ*[*].

Arbor Dianæ capable of being taken out of the vessel in which it is formed.

THE process mentioned by Baumé, which is generally followed for obtaining the peculiar amalgam of mercury and silver, known in chemistry by the name of arbor Dianæ, is not the only one capable of affording those beautiful crystalline figures, that distinguish this curious production. I have obtained the same object by an alteration in the common method, that enables me very easily to remove the metallic arborization from the liquid in which it is formed, and thus to keep it in another vessel unaltered.

Process.

The process is very simple. In the nitric solutions of mercury and silver, both fully saturated, and diluted with the quantity of water directed by Baumé, I suspend 5 or 6 drachms of very pure mercury, tied up in a piece of fine linen doubled. The metallic solutions soon penetrate to the mercury enclosed in the cloth; and we presently perceive clusters of beautiful needles forming round it, and adhering to the nucleus of mercury. These needles gradually increase in bulk, and in a short time extend above an inch in length.

Method of removing it.

When the metallic arborization ceases to increase, the bag loaded with beautiful needly prisms, which appear to me to be tetraedral, is to be taken out; and, by means of the silk thread, with which it was tied up, fastened to a cork. The whole is then to be suspended under a small glass jar, in the midst of which the metallic crystals may be preserved as long as we please. I have a crystallization of this kind in my laboratory, which has retained all its beauty these two years.

Probably the proportions of the amalgam different,

The solidity of the metallic crystals obtained by my method, compared with the weakness of the threads that form the common arbor Dianæ, lead me to suppose, that the proportions of mercury and silver are not the same in the two cases; and I would have endeavoured to ascertain the difference, if Mr. Vauquelin, to whom I have communicated the fact, had not undertaken, to remove every doubt on this head by a comparative analysis.

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[*] Annales de Chim. Vol. LXII, p. 93.

The different configurations of the crystals too may give rise to some interesting researches, which I have not yet had time to pursue.

as are the shapes of the crystals.

SCIENTIFIC NEWS.

Mr. Heinekin having exposed a solution of very pure carbonate of potash to the action of the galvanic pile, found, that in three or four days the liquid next the negative pole had acquired a golden yellow colour; and a very decided smell of oximuriatic acid was perceptible. With the nitrates of silver and of mercury the yellow liquid formed a grumous precipitate; and it completely destroyed the colour of litmus blue, and of ink. The liquid next the positive pole was highly caustic. The conclusions he draws are, that potash and oximuriatic acid are composed of the same principles, or of carbon, hidrogen, and oxigen in different proportions.

Solution of carbonate of potash decomposed by galvanism, and oximuriatic acid formed?

It is a circumstance not a little remarkable, that Mr. Curaudau and Dr. Davy were led to form similar notions of the oximuriatic acid about the same time at Paris and London. From the circumstances of the times it may be presumed, that there could be no communication between them; but it is probable, that, though the merit of discovery is equally due to both these gentlemen, if it be not a fallacy as some suppose, the priority rests with Mr. Curaudau, as his paper was read to the French Institute on the 5th of March, 1810.

Opinion of Mr Curaudau on the simple nature of oximuriatic gas.

The following is one of the experiments, on which Mr. Curaudau founds his opinion. By combining oximuriatic gas directly with nitrate of silver a precipitate is formed, without any oxigen being disengaged; and, as the weight of the precipitate thrown down is proportional to that of the gas employed, he infers, that it is a compound of the muriatic radical and silver. He infers farther, that in this process the hidrogen of the acid disoxidates the silver; and the silver thus disoxidated enters directly into combination with the muriatic radical, so as to form a *muriuret* of silver. Hence we see, why potash in the humid way, and carbon in

Oximuriatic gas forms a union with metallic silver.

the dry, will not decompose this salt: and why, on the other hand, hidrogen so easily effects the reduction of the metal. The proportions assigned by Mr. Curaudau to the muriatic acid are one part of hidrogen to thirty-three of oximuriatic gas.

and with 0·03 of hidrogen composes muriatic acid.

Three meteoric stones.

On the 23d of November, 1810, at half after one in the afternoon, three atmospheric stones fell in the commune of Charsouville, canton of Meung, department of the Loiret. Their fall was accompanied by a series of detonations, which preceded it, and lasted some minutes. The sound of the explosions, to the number of three or four, followed by a rumbling noise occasioned by the echoes, was heard as loud at Orleans as at the place where the stones fell. It is said it was equally loud at Montargis, Salbri, Vierzon, and Blois, at all which places it excited alarm, being supposed to arise from the blowing up of a powder magazine. The explosions must therefore have taken place at a great height.

Circumstances of their fall.

The fall of these stones was perpendicular; and without the appearance of any light, or ball of fire. One fell at Moutelle but has not been found. The other two fell one at Villenai, the other at Moulin Brûlé. All these places are within the distance of a mile. One of the stones weighed about twenty pounds; it made a hole in the ground just large enough for its admission, in a perpendicular direction, driving up the earth to the height of eight or ten feet. The stone was taken out half an hour afterward, when it was still so hot, that it could scarcely be held in the hands. It had a strong smell of gunpowder, which it retained till it was quite cold. The second stone formed a similar hole three feet deep. It weighed forty pounds, and was not taken out of the ground for eighteen hours after its fall, when it was without heat.

The stones described.

These stones were both shapeless masses, irregularly rounded at all their angles. They contain rather more ferruginous globules, than those that fell at l'Aigle, in Normandy; these globules are somewhat larger; and the colour of the stone, when broken, is lighter. They are quickly oxided, very heavy, sufficiently hard to scratch glass, broken with difficulty, and the fracture is irregular and very fine grained. The external crust is a quarter of a line thick, and of a blackish gray colour. The substance of the stone is marked with a few black lines, irregular, very distinct, and from half a line to two lines broad. They traverse it indiscriminately in all directions, like the veins of certain marbles. Does not this seem to indicate, that they existed previous to their fall, and were formed in the same manner as rocks, and not in the atmosphere? The day when these stones fell was remarkably calm and serene; the sun shone as bright as in one of the finest days of autumn; and not a cloud appeared above the horizon.

Directions for sailing to the East Indies &c.

Directions for sailing to and from the East Indies, China, New Holland, Cape of Good Hope, and the interjacent parts, compiled chiefly from Original Journals at the East India House, and from Journals and Observations made during Twenty-one Years Experience navigating in those Seas; by James Horsburg, F. R. S. Part I, published 1809, quarto, 389 full pages with side notes, contents, and a copious index.—Part II, corresponding size and type, 506 pages, just published. Sold by Black, Parry, and Kingsbury.

This valuable publication cannot fail to be of great utility to British navigators, who trade to the southward of the equator, as well as those belonging to his Majesty's navy. Exclusive of sailing directions and local descriptions of winds, weather, currents, ports, headlands, islands, coasts, dangers, &c., the geographical situations of all the particular headlands, islands, ports, and dangers, are stated from actual observations of sun, moon, and stars, or by good chronometers. The necessity of a work of this nature has long been known to navigators; as, former directories having been compiled from a mass of heterogeneous and *very incorrect* materials, obtained when ships were navigated by dead reckoning, prior to the application of marine chronometers and lunar observations to nautical science; and these directories, for the greater part, having been generally transcribed from each other for nearly a century up to the present time: they are constantly fraught with errour, and of little use in the present improved state of navigation.

Upon this work the author has bestowed nearly five years of almost constant labour, in order to render it as correct as possible, conformably to the important end he had in view, which was the security of the lives and property of numbers in a great commercial nation. How far this end has been attained, scientific and naval men can justly appreciate.

Medical and Chemical Lectures, St. George's Hospital, and George Street, Hanover Square.

Medical and chemical lectures.

These Medical Lectures will recommence as usual in the first week of October, at eight in the morning; and the Chemical at a quarter after nine, at No. 9, George-street, Hanover-square.

Clinical Lectures are given on the cases of patients registered in St. George's Hospital, every Saturday morning at nine o'clock; by George Pearson, M.D. F.R.S., sen. physician to St. George's Hospital; of the College of Physicians; honorary Fellow of the Imperial Medico-chirurgical Academy of St. Petersburgh, &c.

Lectures on Surgery, and on Physiology.

Lectures on surgery and physiology.

Mr. A. Carlisle F.R.S. F.L.S. professor of Anatomy in the Royal Academy, and surgeon to the Westminster Hospital, will begin his Course of Lectures on the Art and Practice of Surgery, on Tuesday, October 8, at eight o'clock in the evening, at his house in Soho-square.

The subject will be continued on Tuesdays, Thursdays, and Saturdays, at the same hour.

The Diseases and Accidents allotted to the province of Surgery will be fully treated of, and illustrated by Cases from the Lecturer's experience. The different Operations will be demonstrated, and the Anatomy of the Parts explained.

These Lectures combine Views of the Natural History, Physiology, and Pathology of the Human Body, calculated to illustrate the several Processes of Healing, and to afford a compendious View of the Animal Economy.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1811.

ARTICLE I.

On a Property of the repulsive Forces, that act on Light: *by Mr.* MALUS[*].

New properties of reflected light.

In my last paper[†] I announced, that light reflected from the surface of transparent bodies acquires new properties, which distinguish it essentially from that which emanates directly from luminous bodies.

Investigation continued.

I have since continued my researches on the same subject; and, subjecting the results of my experiments to calculation, I have arrived at some remarkable consequences, which tend to elucidate the mode of action of substances on light.

Reflected light acquires the properties of light doubly refracted.

The partial reflection from transparent substances prevented:

I had observed, that, when the light is reflected under a certain angle by the surface of a diaphanous body, it acquires the properties of the rays, that have been subjected to double refraction: and, setting out with this remark, I contrived to modify the rays of light by simple transparent substances, so that they entirely escaped the partial reflection, which is commonly observed at the surface of these substances. I cause any number of these substances to be traversed by a solar ray, without any of its particles being reflected; which furnishes means of measuring with accuracy the quantity of light, that these substances absorb; a problem, which the partial reflection had rendered impossible to be solved.

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[*] Mém. de la Soc d'Arcueil, vol. II, p 254.

[†] Journal, p. 95.

and from opake polished bodies.

The light that has undergone this modification comports itself in a similar manner with opake polished bodies. Under determinate angles it ceases to be reflected, and is totally absorbed, while within and beyond these angles it is in part reflected from the surface of these bodies.

Direct ray on an unsilvered glass.

When a solar ray is made to fall on a polished glass, that is not silvered, this ray is in part reflected at the first and second surface, and its intensity increases with the angle of incidence, reckoning from the perpendicular: in other words, it is so much the greater, in proportion as the ray is more inclined to the reflecting surface.

Light previously reflected follows a different law.

But if the direct light be subject to this law of intensity, that which has been already reflected follows a very different law, when it is reflected anew by a second glass. In certain directions, instead of increasing in intensity with the angle of incidence, on the contrary it diminishes; and, after having attained a certain minimum, begins to increase according to the same law as the direct light. These minima are relative either to the inclination of the ray to the reflecting surfaces, or to the angles which these surfaces form with each other, so that the light reflected by the second glass is a function of these three angles. This function has an absolute minimum; that is to say, a point at which the intensity of the light reflected by the second glass is altogether null. Calculation has led me directly to the circumstances, that produce this minimum; and I have verified it by a very simple experiment, which I shall proceed to describe.

Angle at which all the ligh reflected by one glass is absorbed by a second.

If we take two glasses inclined to each other at an angle of 70° 22′: if we then conceive between these two glasses a line making with each an angle of 35° 25′, every ray reflected by one of the glasses parallel to this line will not be reflected anew by the second; it will penetrate it, without any of its particles experiencing the action of the repulsive forces, that produce the partial reflection. Within or beyond the angles I have mentioned, the phenomenon will cease to take place; and the farther we go from these limits, on either side, the greater will be the quantity of light reflected.

This property retained or left under different circumstances.

This faculty of entirely penetrating transparent bodies, which the light has acquired by its first reflection, it will lose or retain in various circumstances, which I have studied; and thus I have been led to the following law, according to which this singular phenomenon is effected.

Law of this phenomenon.

If a second glass be made to turn round the first reflected ray, *a*, constantly making with it an angle of 35° 25′; and if in a plane perpendicular to this ray we conceive two lines, one, *b*, parallel to the first glass, and the other, *c*, parallel to the second; the quantity of light reflected by the second glass will be proportional to the square of the cosine of the angle included between the lines *b c*: it is at its maximum when these lines are parallel, and null when they are perpendicular. So that the limits of the phenomenon are relative to three rectangular axes, *a*, *b*, *c*, one of which is parallel to the direction of the ray, another to the first reflecting surface, and the third is perpendicular to the two former.

Metallic mirror substituted for the second glass.

For the second glass let us substitute a metallic mirror, and call the rectangular axes of the second ray, analogous to the axes *a*, *b*, *c* of the first, *a′*, *b′*, *c′*. If this ray be received on a polished but unsilvered glass, which makes with it an angle of 35° 25′, we shall perceive the following phenomena, which are independant of the angle of incidence on the metallic mirror. If *b′* be parallel to *b*, that is, if the metallic mirror be parallel to the axis *b*, the ray it reflects will retain its properties with respect to a glass situate parallel to the axis *c′*; it will penetrate it entirely. If *b′* be parallel to *c*, the reflected ray will retain its properties for a glass parallel to the axis *b′*.

In the intermediate positions, the quantity of light, that will have retained its property for a glass parallel to the axis *b′*, is proportional to the square of the sine of the angle comprised between the axes *b′ b*; and that which has retained its property with respect to a glass parallel to the axis c' is proportional to the square of the cosine of the same angle.

When the metallic mirror makes equal angles with the axes b and c, b' makes an angle of 45° with each; and then the light comports itself in the same manner on a glass parallel to the axis b, or to the axis c'; it seems, in this case, to have resumed all the characters of direct light.

Ray from the metallic mirror dissected by calcareous spar.

If the ray reflected by the metallic mirror be dissected by means of a crystal of calcareous spar, in disposing its principal section parallel to the plane of reflection, the proportion of the intensities of the ray refracted extraordinarily and the ordinary ray is equal to the square of the tangent of the angle included between the two axes, b, b'.

Light reflected several times from metallic mirrors.

If the light be made to undergo several reflections from metallic mirrors, before subjecting it to the action of a second transparent body, the phenomena are analogous to those I have mentioned. If the axis b' of the second ray be parallel to the axis b or c of the first; if the axis b'' of the third be parallel to the axis b' or c' of the second; and so for the rest; the property of the light already laid down will be in no respect altered: but if these axes be inclined to one another, it will be divided with respect to the two consecutive mirrors, according to the law I have mentioned.

Reflected light received on black marble.

If the surface of a polished opake substance, as black marble, be made to turn round the axis c of the first reflected ray, the reflected light will be seen to diminish to a certain point, at which it is null, and beyond which it begins to increase.'

Ordinary phenomena of optics explicable on the hypothesis of Huyghens, or of Newton:

All the ordinary phenomena of optics may be explained either on the hypothesis of Huyghens, who supposed them to be produced by the vibrations of an ethereal fluid; or agreeably to the opinion of Newton, who supposed them to be produced by the action of bodies on luminous particles, considered as themselves belonging to a substance obeying the attractive and repulsive powers, that serve to explain other physical phenomena. The laws respecting the course of rays in double refraction too may be explained on either hypothesis. But the observations I have related prove, that the phenomena of reflection are different at the same

but those here mentioned not reconcilable with that of Huyghens.

same angle of incidence, which cannot take place on the hypothesis of Huyghens: for we must necessarily conclude from them, not only that light is a substance obedient to the forces that set other substances in action, but also that the form and arrangement of its particles have great influence on the phenomena.

All the phenomena deducible from a single law.

If we transfer to the luminous particles the three rectangular axes, *a*, *b*, *c*, to which the phenomena I have described are referrible; and if we suppose, that, the axis *a* being still in the direction of the ray, the axis *b* or *c*, from the influence of the *repulsive powers*, becomes perpendicular to the direction of these powers; then all the phenomena of total reflection, and of partial reflection, and the most extraordinary circumstances of double refraction, become consequences of one another, and are deducible from this single law, namely, that;

The law.

If we consider, in the transference of the luminous particles, their motion round their three principal axes, *a*, *b*, *c*; the quantity of particles, the axis *b* or *c* of which becomes perpendicular to the direction of the repulsive forces, will always be proportional to the square of the cosine of the angle, which these lines will have to describe round the axis *a*, to take this direction; and reciprocally, the quanquantity of the particles, the axis *b* or *c* of which will approach the nearest possible to the direction of the repulsive forces, will be proportional to the square of the sine of the angle, which these lines will have to describe in their rotation round the axis *a*, to arrive at the plane, that passes through this axis and the direction of the forces.

In the case of double refraction, and when we consider the phenomena, that are exhibited by two contiguous crystals, we may express this law in the following manner.

Law in the case of double refraction.

If we conceive a plane passing through the ordinary ray and the axis of the first crystal, and a second plane passing through the extraordinary ray and the axis of the second crystal, the quantity of light proceeding from the ordinary refraction of the first, and refracted ordinarily by the second, is proportional to the square of the cosine of the angle comprised between the two planes abovementioned; and the

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axis c' is proportional to the square of the cosine of the same angle.

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Reflection.

With regard to reflection, if we consider, for example, a ray reflected by one glass, with which it makes an angle of 35° 25′. and falling on a second glass at the same angle, the angle comprised between the two surfaces being in other respects arbitrary: we must conceive a plane perpendicular to the first glass, and another perpendicular to the second, passing through this reflected ray; and the quantity of light reflected by the second glass will be proportional to the square of the cosine of the angle comprised between these two planes.

I shall confine myself to a few examples of the application of this law.

Examples of the application of this law.
Example 1.

When a ray is reflected by the surface of a glass at an angle of 54° 35′, we find, that all its particles are disposed in the same manner; since, if we present perpendicularly to this ray a prism of crystallized calcareous spar, the axis of which is in the plane of reflection, all its particles will be refracted in a single ordinary ray, none being refracted extraordinarily. In this case the analogous axes of these particles are all parallel, since they all comport themselves in the same manner. Let us call the axis of these particles, which are perpendicular to the plane of reflection, *b*. All the particles, of which the axis *c* was perpendicular to that plane, have penetrated the transparent body. If therefore we present to the particles reflected, and under the same angle, a second glass parallel to their axis *c*, they will be found similarly circumstanced with those, which could not be reflected by the first; the ray therefore will penetrate this second glass entirely. In fact, experiment shows, that, under these circumstances, all the particles escape the forces of reflection.

Example 2.

When we place two rhomboids of calcareous spar on one another, so that their principal sections are parallel, a solar ray parallel to these principal sections produces but two emergent rays; those which arise from the ordinary and extraordinary refraction of the first crystal being refracted each in a single ordinary or extraordinary ray by the second. In fact in this case it may be conceived, that, whether the axes of the crystals be parallel, or placed in opposite directions, every ray issuing from the first crystal parallel to its principal section is not divided by the second, for its movement takes place round the axis *b* or the axis *c*; and we have seen by the phenomena of reflection, that, whenever the movement takes place round these axes, the ray is not altered; all the particles preserving the parallelism of their similar axes. The rotation round the axis *a* being the only one, that changes the respective positions of the axes of the particles of a given ray.

Example 3.

When the incident ray makes any angle whatever with the principal sections, the rays that proceed from the double refraction of the first crystal are divided into two by the second, so that we then obtain four emergent rays. In this circumstance however there are two different cases, in which the phenomena are very distinct: that in which the axes of the crystals are parallel, and that in which they are in opposite directions. When the axes are parallel, a very vivid light must be employed, and the plane of incidence must be removed to a sensible distance from that of the principal sections, to be able to perceive the rays refracted ordinarily by one crystal and extraordinarily by the other. In fact, agreeably to the theory, the maximum of intensity of these two rays is not the thirtieth part of that of the ray, which proceeds from the ordinary refraction of the two crystals; which has led those who have written on this subject to imagine, that, when the principal sections and the axes are parallel, the light comports itself in the same manner as in the principal section, whatever be the direction of the incident ray: but if we employ a vivid light, under suitable circumstances, observation accords perfectly with the theory. The phenomenon is much more evident, when the axes are in opposite directions.

Extraordinary refraction.

The extraordinary refraction is produced by a repulsive force, the action of which is proportional to the square of the sine of the angle included between the axis of the crystal, and the principal axis, *a*, of the luminous particle. All the particles, of which the axis *b* is perpendicular to this force, are refracted ordinarily; and all those of which the axis *c* is perpendicular to it, are refracted ordinarily. The particles refracted ordinarily, that escape the repulsive force, are in the same case with those, that escape reflection in the first class of facts I have described.

Double reflection. The phenomena of double reflection at the second surface of transparent crystals are analogous to those of the refraction in two crystals, the principal sections of which are parallel, and their axes in opposite directions; with the addition of this property common to all diaphanous bodies, that, when the reflecting face is parallel to the axis *c* of the luminous particles, the reflection at a determinate angle is null.

General remarks. Thus, without the knowledge of this singular property of transparent substances, the most extraordinary part of the phenomena of double refraction would have remained inexplicable.

I shall not enter more largely into the particulars of the application of the theory I have brought forward, but shall content myself with saying, that it refers to one source a number of facts, which seemed to have no analogy to each other, and the want of connexion in which rendered it almost impracticable to measure them.

I do not pretend to point out the cause of this general property of the repulsive powers that act on light; I merely exhibit the means of connecting the phenomena with each other, of ascertaining them before hand by calculation, and of measuring them with accuracy: at the same time in referring the figures of the luminous particles to three rectangular angles, as those of an octaedron would be, I anticipate nothing respecting the real figure of these particles; but I present the result as a consequence of the calculation, to which I have been led by the analysis of the phenomena that I have observed.

II.

Experiments on the Production of Sound in Vapour: by Mr. Biot*.

AN infinite number of experiments have been made on the manner in which sound is produced and transmitted in different mediums. It has been shown, that it is neither formed nor transmitted in a vacuum; and its transmission through solids and liquids has been examined: but no one, I believe, has yet thought of making these experiments in vapour. Such an inquiry however is well calculated to excite our curiosity; for, setting out with the results that experience has made known with respect to the constitution of the vapour that fills a given space, and applying to them the mathematical principles on which the laws of the minute vibrations of elastic fluids are usually founded, it is evident, that no sound should be produced in vapour.

Production and transmission of sound in vapour not yet examined.

No sound should be produced in vapour.

In fact it is shown, by the experiments of De Luc, Saussure, and Dalton, that the quantity of vapour of water, or of any other liquid, that is formed in a vacuum, depends only on the dimensions of that vacuum and the temperature: so that, if this vapour have an elasticity capable of sustaining the manometer at a certain height, and you compress it slowly, so as to oblige it to occupy a smaller space, the elasticity will not be increased by this compression, as that of a permanent gas would be; but a portion of the vapour will return to the liquid state, without any variation of the manometer; and only so much will remain, as is adapted to the new limits, to which the vacuum is reduced. The reverse will happen, if the space be enlarged instead of diminished: a new quantity of vapour will be formed to fill it, but without any change in the elasticity, or in the manometer. These results have been completely established by the learned gentlemen I have mentioned, and we may easily convince ourselves of their accuracy. It is sufficient to introduce into a barometer a small quantity of any liquid; and to measure the height at which the mercury stands,

Properties of vapour.

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* Mém. de la Soc. d'Arcueil, vol. II, p. 94. Read to the Institute, October the 12th, 1807.

after it is depressed by the elasticity of the vapour formed. If we then raise or lower the external level of the mercury, the interior column will rise or fall exactly as much in the tube; and thus, according as the space remaining at the top of the tube is diminished or increased, a part of the vapour will be precipitated, or fresh vapour will be formed: but, the temperature remaining the same, the elasticity will not alter.

Vibrations of a sonorous body in it.

Now let us suppose, that a sonorous body begins to vibrate in such a medium; each of its vibrations will diminish the space in one direction, and increase it in the opposite. Thus on one side there will be a small quantity of vapour reduced to the liquid state, and on the other a small quantity of liquid will assume the state of vapour. These condensations and dilatations will take place close to the sonorous body in the small extent of its vibrations, but will not be produced beyond this. Thus the motion will not be propagated through the rest of the fluid mass, and consequently the sound will not be transmitted.

If we suppose these to disengage heat, sound should be produced in it.

Let us next suppose, that the sonorous body, in compressing the vapour by its rapid vibrations, disengages from it mechanically a certain quantity of heat. This supposition is by no means improbable, for we know, that vapour gives out a great deal of heat in its condensation. The vapour of water, for example, according to the experiments of Watt, in returning to the liquid state gives out a quantity of heat, that is capable of raising the temperature of the water thus produced to 525° [977° F.]. If we take this circumstance into consideration, the effects of the sonorous body on vapour will no longer be the same: the portions it compresses will preserve their elastic state, notwithstanding the diminution of the space, in consequence of the heat evolved, which instantly increases their elasticity. On the contrary, in the portion dilated the diminution of temperature, preventing a new evaporation, diminishes the elasticity. The phenomena produced near the sonorous body therefore are of the same nature, as if the vapour became a permanent gas. There will be successive and momentary augmentations and diminutions of elasticity, the effect of which will be transmitted step by step throughout the whole of the fluid mass, so as to permit sound to be produced and transmitted in it.

This may be brought to the test of experiment.

Experiments on the production of sound in vapour therefore are calculated to decide the question, whether heat be really evolved in an aeriform medium by the effect of the vibrations of sonorous bodies, as we see it in general extricated by any rapid compression. Thus we may subject to decisive proof the ingenious idea of Mr. Laplace, by which he has found means of reconciling the mathematical theory of the transmission of sound in air with the results of experience, taking into account the heat evolved: for, if the effect he supposes do not take place, the vibrations of sonorous bodies in vapour should not produce any sound; and, if they do produce sound, it can be only in consequence of the evolution of heat.

Sound was produced in vapour.

Induced by these motives, I made some experiments on the subject, which completely succeeded. I then repeated them in a more perfect manner, in the philosophical apartments at Arcueil, with my friend Amadeus Berthollet. Mr. Berthollet, and Mr. Laplace were present at these experiments, and themselves verified the facts I am going to relate.

Apparatus described.

We took a glass globe that held 36 litres [near 38 wine quarts]. Its orifice was closed by a well made cock, so that a vacuum might be made in it, which it preserved with great accuracy. To this cock another could be screwed; so that, by pouring a liquid into the space between them, and closing both, this portion of liquid could be afterward introduced into the globe, without admitting any air from without. The sonorous body was a small bell, suspended within the globe by a slender string fastened to the lower cock.

Experiment. In a vacuum no sound.

A vacuum was first made within the apparatus to the greatest nicety, and even so as to exhaust a great part of the hygrometrical water, that might have existed in the globe, which however was very dry. Then, holding the globe by the cock, we set the bell in motion, so as to satisfy ourselves, that the clapper struck very forcibly against the sides: yet, with all the attention we could bestow, even close to the globe itself no sound could be perceived; so that there was no perceptible sound in a vacuum, agreeably to the experiments of Hawksbee, and all other philosophers.

We

In aqueous vapour sound produced

proportional to its density.

We then introduced into the globe, in the way I have described, a small quantity of water, part of which was converted into vapour. The sound immediately began to be perceptible, though the density of this vapour was extremely small, the temperature being only 19° [66·2° F.]. To increase it, an excess of water was admitted into the globe, and it was placed in a stove at the temperature of 46° [114·8° F.]. The sound then became very perceptible: it could be heard without stooping down to the globe, and even out of the stove through the door. Some water still remained in the globe, so there can be no doubt, that the sound was produced and transmitted in the aqueous vapour.

When the globe was taken out of the stove, the temperature quickly fell: a great part of the vapour therefore, which had been raised in consequence of the temperature, was necessarily precipitated; and accordingly the sound appeared very evidently diminished.

In vapour of alcohol sound louder.

Without any alteration in the apparatus, we introduced the same quantity of alcohol, as we had before of water. The specific gravity of this alcohol was 0·823. The vapour from this mixture possessed of course greater density and elasticity than that of water at the same temperature; and accordingly the sound was much more perceptible: it was heard from one extremity to the other of the rooms that form the philosophical apartments at Arcueil. Sound therefore is produced and transmitted in the vapour of alcohol.

Experiment in vapour of ether.

Distance at which the sound was heard in atmospheric air.

As a last experiment we tried the vapour of ether. This particularly excited our curiosity, on account of its great elastic force and density, which are known to be very considerable; two circumstances, that must contribute to increase the intensity of the sound. We begun with drying the globe, because the moisture would have diminished the tension of the ether; and then allowed the atmospheric air to enter freely, till it was in equilibrio with the external pressure, which was 0·7613 [29·951 inch.]; and, carrying it into a long walk in the garden, we found, that the sound of the bell was sensible to the distance of 145 met. [158·5 yd.]: beyond this it was so faint, that the perception of it was not sufficiently certain. The temperature was 17·75° [63·95° F.]. Having measured by this experiment the intensity of the sound produced in atmospheric air, we again made a vacuum in the globe, and introduced into it a sufficient quantity of sulphuric ether, to leave a surplus above what the temperature could convert into vapour. The specific gravity of this ether was 0·759. The elastic force of its vapour, measured by introducing it under a barometer freed from air, was 0·3549 met. [13·963 inches], at the temperature of 17·75° [63·95° F.]. The globe being filled with this vapour, it was carried to the same place as in the preceding experiment; when we found, that the sound was perceptible to the distance of 131·5 met. [143·7 yards]. This conclusively proves in the most convincing manner, that sound is produced and transmitted in vapour, as well as in a permanent gas. But we have proved, that this can take place only from the effect of instantaneous variations of temperature, occasioned by the vibrations. It evidently follows therefore, that this cause really exists; and that, according to the judicious remark of Mr. Laplace, it becomes indispensable for us to pay attention to it in the mathematical theory of the propagation of sound; though we cannot directly verify it by the application of the thermometer, because this instrument can no more be affected by these successive and momentary variations of heat, than the barometer is by the momentary variations of elasticity, that take place in the production of sound, and of which every one notwithstanding acknowledges the existence.

Ether introduced

Elasticity of its vapour.

Distance at which the sound was heard.

This proves the momentary variations of temperature caused by vibrations according to the theory of Laplace.

III.

Experiments to prove, that Fluids pass directly from the Stomach to the Circulation of the Blood, and thence into the Cells of the Spleen, the Gall Bladder, and Urinary Bladder, without going through the Thoracic Duct. By EVERARD HOME, *Esq. F. R. S**.

HAVING on a former occasion laid before the Society some experiments, to prove, that fluids pass directly from

Fluids pass from the stomach into the blood,

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* Philos. Trans. for 1811, p. 163.

the cardiac portion of the stomach, so as to arrrive at the circulation of the blood without going through the thoracic duct, the only known channel by which liquids can arrive there; the present experiments are brought to confirm that opinion; but in stating them, I wish to correct an errour, I was led into, in believing that the spleen was the channel, by which they are conveyed.

but not through the spleen.

The passage might be found by tying the thoracic duct.

At the time I made my former communications*, I was conscious, that the facts I had ascertained were only sufficient to open a new field of inquiry; but as I might never be able to make a farther progress in an investigation, beset with so many difficulties, I thought it right to put them on record. Since that time I have lost no opportunity of devising new experiments to elucidate this subject; and the circumstance of Mr. Brodie, the assistant of my philosophical as well as professional labours, having tied the thoracic duct in some experiments which will come before the Society, suggested to me the idea, that, if the thoracic duct was tied, and proper experiments made, there could be no difficulty in ascertaining whether there was any other channel between the stomach and the circulation of the blood.

With this view I instituted the following experiment, which was made on the 29th of September, 1810, by Mr. Brodie, assisted by Mr. William Brande and Mr. Gatcombe. I was unavoidably prevented from being present during the time of the experiment.

Exp. 1, on a rabbit.

Exp. 1. A ligature was passed round the thoracic duct of a rabbit, just before it enters at the junction between the left jugular and subclavian veins: an ounce of strong infusion of rhubarb was then injected into the stomach. In three quarters of an hour some urine was voided, in which rhubarb was distinctly detected, by the addition of potash. An hour and a quarter after the injection of the rhubarb the animal was killed: a dram and half of urine was found in the bladder highly tinged with rhubarb, and the usual alteration of colour took place on the addition of potash. The coats of the thoracic duct had given way opposite the middle dorsal vertebra, and nearly an ounce of chyle was found effused into the cavity of the thorax, beside a considerable quantity in the cellular membrane of the posterior mediastinum. Above the ruptured part the thoracic duct was entire, much distended with chyle; and on tracing it upwards, the termination of the duct in the vein was found to be completely secured by the ligature. The lacteal and lymphatic vessels had given way in several parts of the abdomen, and chyle and lymph were extravasated underneath the peritoneum.

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* See Journ. vol. XX, p. 374, and XXI, 108.

Infusion of rhubarb used as the most sensible test.

In this and the following experiments the infusion of rhubarb was employed in preference to the prussiate of potash, in consequence of its having been found in those I formerly made, that one drop of tincture of rhubarb could be detected in half an ounce of serum, and nothing less than a quarter of a grain of prussiate of potash in the same quantity could be made to strike a blue colour when the test was added.

Exp. 2, on a dog.

Exp. 2. The experiment was repeated upon a dog. In this I was assisted by Mr. Brodie, Mr. William Brande, Mr. Clift, and Mr. Gatcombe. After the thoracic duct had been secured, two ounces of strong infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine in the bladder, on the addition of potash, became deeply tinged with rhubarb. The bile in the gall bladder, by a similar test, was found to contain rhubarb. The lacteal vessels in several parts of the mesentery had burst, and chyle was extravasated into the cellular membrane; the thoracic duct had given way in the lower part of the posterior mediastinum, and chyle was extravasated. Above the ruptured part the thoracic duct was much distended with chyle; it was readily traced to the ligature, by which it was completely secured.

The thoracic duct not the passage

These experiments appeared to establish the fact, that the thoracic duct was not the channel through which the infusion of rhubarb was conveyed to the circulation of the blood, and it now became easy to ascertain, whether it passed through the spleen, by extirpating that organ, and repeating the last experiment.

On the 21st of October, 1810, the following experiment was made with the assistance of Mr. Brodie, Mr. Clift, Mr. Gatcombe, and Mr. Money.

Exp. 3, on a dog. The thoracic duct tied, and the spleen extirpated.

Exp. 3. The thoracic duct near its termination was secured in a dog, whose spleen had been removed four days before, and three ounces of infusion of rhubarb were injected into the stomach. In an hour and half the dog was killed, and the urine was found strongly impregnated with rhubarb; and on examination, the thoracic duct was found to be completely secured by the ligature. Several of the lacteals had burst, but the duct itself had not given way; it was greatly distended with chyle and lymph.

The spleen not the passage.

By this experiment it was completely ascertained, that the spleen is not the channel through which the infusion of rhubarb is conveyed into the circulation of the blood, as I had been led to believe, and therefore the rhubarb, in my former experiments detected in the spleen, must have been deposited in the same manner as in the urine, and in the bile.

In the next experiment the termination of the thoracic duct on the left side, and the lymphatic trunk of the right side, both secured.

The detection of this errour made me more anxious to avoid being misled respecting the thoracic duct; and therefore, although there was little probability that the infusion of rhubarb could have passed into the lymphatic vessels, which open into the blood vessels of the right side of the neck, I thought it right, before I proceeded farther, to repeat the experiment, securing the termination of the thoracic duct on the left side, and the lymphatic trunk of the right side, where it empties itself into the angle between the jugular and subclavian vein. This was done on the 28th of October, 1810, with the assistance of the same persons as in the last experiment.

Exp. 4, on a dog.

Exp. 4. The thoracic duct of a dog was tied, as in the former experiment; in doing it the duct was wounded, and about a dram of chyle flowed out; the lymphatic trunk of the right side was then secured. After this, three ounces of infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine and the bile were found distinctly impregnated with rhubarb. On opening the thorax, some absorbent vessels, distended with lymph, were seen on the right side of the spine, entering an absorbent gland on the second dorsal vertebra, and the vasa efferentia from the gland were seen uniting with other absorbent vessels, and extending towards the right shoulder, where they formed a common trunk with the absorbents from the neck and axilla; this trunk was found included in the ligature. The thoracic duct was moderately distended with a mixture of chyle and lymph; in tracing it upwards, an opening was seen in it immediately below the ligature, through which the contents readily passed out when pressure was made on the duct: above this opening the duct was completely secured by the ligature. Nearly a dram of the fluid contained in the thoracic duct was collected and tested by potash, but there did not appear to be any impregnation of rhubarb.

Exp 5, on a dog.

Exp. 5. The last experiment was repeated on another dog, on the 21st of January, 1811, with the assistance of Mr. Brodie, Mr. W. Brande, Mr. Clift, and Mr. Gatcombe. The dog was killed an hour after the thoracic duct and lymphatic trunk had been secured, and the infusion of rhubarb had been injected into the stomach.

In tying the right lymphatic trunk, a lymphatic vessel from the thorax going to join it was wounded, from which chyle flowed out in considerable quantity during the whole time of the experiment; a short time before the dog was killed some of it was collected, but on testing it with potash no rhubarb was detected in it.

The urine was found impregnated with rhubarb, as was also the bile from the gall bladder; but both in a less degree than in the last experiment. The lacteal vessels and mesenteric glands were much distended with chyle; and on cutting into the glands chyle flowed out in considerable quantity. Some of this was collected and tested with potash, but showed no evidence of rhubarb being contained in it. The thoracic duct was much distended; it was traced to the ligature, and was found to be completely secured.

Lymphatic vessels from the right side of the posterior mediastinum were seen extending towards the ligature, that had been tied on that side; they were nearly empty; and the trunk formed by the junction of these with the lymphatic vessels from the right axilla, and from the right side of the neck, was seen distinctly included in the ligature.

Some rhubarb found in the spleen.

While Mr. Brodie was tracing the thoracic duct, Mr. William Brande was making an infusion of the spleen, and showed but none perceptible in the liver.

showed me a section of it, in which the cells were larger, and more distinct, than I had ever seen them in a dog. There was a slight tinge of rhubarb in the infusion from the spleen. A similar infusion was made of the liver; but the quantity of blood contained in it being much greater than in the spleen, the appearance was not sufficiently distinct to decide whether it contained rhubarb or not. These experiments appear completely to establish the fact, that the rhubarb did not pass through the thoracic duct, and therefore must have got into the circulation of the blood by some other channel. They likewise completely overturn the opinion I had adopted of the spleen being the medium by which the rhubarb had been conveyed, and show that the spleen answers some other purposes in the animal economy.

The rhubarb probably deposited in the spleen in the form of a secretion.

The rhubarb found in the spleen does not arrive there before it enters the circulation, it is therefore most probably afterwards deposited in the cells in the form of a secretion. That the rhubarb goes into the circulation is proved by my former experiments, in which it was detected in the splenic vein. The prussiate of potash is hardly to be discovered in the blood of a living animal, since the proportion which strikes a blue colour on the addition of solution of iron, is greater than the circulating fluids can be expected to contain at any one time, as it goes off by the secretions nearly as fast as it is received into the blood vessels. In a moderately sized ass more than two drams must be dissolved in the blood before its presence there can be detected.

That the fluid contained in the cells of the spleen is secreted there, is rendered highly probable, since it is most abundant while the digestive organs are employed, and scarcely at all met with when the animal has been some time without food. The great objection to this opinion is, there being no excretory duct but the lymphatic vessels of the spleen; these however are both larger and more numerous than in any other organ; they are found in the ass to form one common trunk, which opens into a large gland on the side of the thoracic duct, just above the receptaculum chyli; and when the quicksilver is made to pass through the branches of this gland, there is a trunk equally large on the opposite side, which makes an angle, and then terminates

The lymphatics of the spleen probably form its excretory duct in the thoracic duct. This fact I ascertained at the Veterinary College, assisted by the deputy professor Mr. Sewell, and Mr. Clift. These lymphatic vessels are equally large as the excretory ducts of any other glands, and therefore sufficient to carry off the secretion formed in the cells of the spleen; and where a secretion is to be carried into the thoracic duct, it would be a deviation from the general plan of the animal economy, were any but lymphatic vessels employed for this purpose.

conveying its secretion into the thoracic duct.

It is a strong circumstance in favour of the secretion being so conveyed, that in the last experiment, the lacteals and cells of the spleen were unusually turgid, being placed under similar circumstances, the thoracic duct being so full as not to receive their contents.

The purposes that are answered by such a secretion from the spleen into the thoracic duct cannot at present be ascertained.

IV.

Of the mechanical Powers in the Leaf Stalks of various Plants.

In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

IN pointing out what appears to me to be the perfection of mechanism in the hairs of plants I have by no means exhausted the subject, but rather begun it. The mechanism of botany, though not yet familiarized to our ideas, is not the less beautiful, or true. As I have introduced it, so I shall continue to exhibit specimens of it, showing, that there is not a part of a flower, leaf, or stem, that is not managed by mechanical means. This is admirably depicted in the leaf stalk, which I shall make the subject of the present letter; as Mr. Knight, in his view of it, has given only one sort of peduncle, without describing the increased

Mechanical power in plants:

depicted in the leaf-stalk.

size of the part that joins the stem, which at one time of the year is but little larger, and shows the gatherer* but poorly.

There appears a regular gradation of mechanism in this part of all plants, from those which, having the leaves perfectly sessile, are fastened in such a manner to the stem, as to be absolutely incapable of turning, or moving in any manner, to those plants, the leaves of which move with a touch, and the mechanism of which I have before described in the *mimosa sensitiva*†.

Mechanism increasing from the firs to the mimosas.

I have already with indefatigable pains traced this gradation through 130 genera of plants, differing as much as possible, selecting in each a few to illustrate this truth, and in which the mechanism increases gradually from the firs, the leaves of which move not, and have therefore no spiral wire, to the mimosa, which has it knotted and turned over balls.

Most plants have gatherers.

The first degree of motion in the peduncle is caused by the simple spiral wires in their cases passing into every diminutive vessel in the leaf. The motion is then as simple as the means, and the leaf is merely drawn nearer, or falls farther from the stalk: but when the spiral wire is doubled or crossed, there appears some diversity of motion, by the leaf not only advancing and retiring, but being able to be drawn on one side. The next gradation is shown by the increase of the peduncle next the stalk, and this increase I have ventured to call the gatherer, because it contracts and dilates to favour the spiral wire. When this is found double, that is, adjoining the leaf, as well as the stem, the motion is very greatly increased, since each of them moves through the third of a circle, as I shall presently show. When there appears a ball within the gatherer, the leaf generally proves to be one of those compound leaves, which close as the evening advances. The gradation from this to the mimosa, or those leaves which move with a touch, seems effected by more balls, and by the spiral wire being knotted in a more complicated manner. It would have been curious to show one of each of these specimens, which I have drawn for myself; but they would take up too much room, Sir, in your Journal: I shall therefore give only a specimen of what I mean by a gatherer; and a representation of the leaf-stalk dissected.

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* The name by which I distinguish the increased part of the peduncle.

† See Journal, vol. XXIV, p. 160.

The beautiful arrangement of leaves.

No person can have examined a tree with attention, without observing the beautiful arrangement of its leaves; the exquisite manner in which they are prevented from obstructing the light, or keeping the air from each other, and the various curious contrivances (especially with large leaves) manifested in raising or depressing them, so as to prevent their throwing too deep a shade on each other, and on those that are beneath them. It is to the gatherers alone they are indebted for this, to the power the two ends of the peduncle have of turning through the third of a circle, that they are able to place themselves in this manner, and arrange their leaves in such beautiful order, so conducive to their benefit and future health.

Explanation of the plate.

The peduncle may generally be divided into three parts, and, if it has any mechanism to manage, which it is seldom without, it is always found in two of these parts, that which joins the peduncle to the stem, and that which unites the leaf to the peduncle. Pl. V, fig. 1, is a drawing of the peduncle of the liburnum or cytisus. A B are the two gatherers; and C D are the same extremely magnified, and dissected; it is easy to see, that the spiral wire being much contracted may draw these into various figures, according as it is tight or loose within the gatherers, as it is at *e e*, and may turn them three parts of a circle; and thus make the leaf or leaf-stalk measure a very extensive circumference; and by this means accommodating its neighbour, and placing itself in the most eligible situation, not only for its leaf, but for the buds which are trusted to its care, and generally in the axilla of its peduncle. The gatherers at both ends appear, when much contracted, like a screw at the exterior, and sometimes they are so bent as to be doubled, but at another time you will hardly be able to see that they do gather, so various is their figure. I shall now show a specimen of a leaf-stalk, which comes nearer in gradation to the sensitive plant, one of the medicagoes, differing little from the trifoliums, and many of the diadelphian plants.

Description of the medicago polymorpha.

Fig.

Formation of the balls.

Form of the everlasting pea.

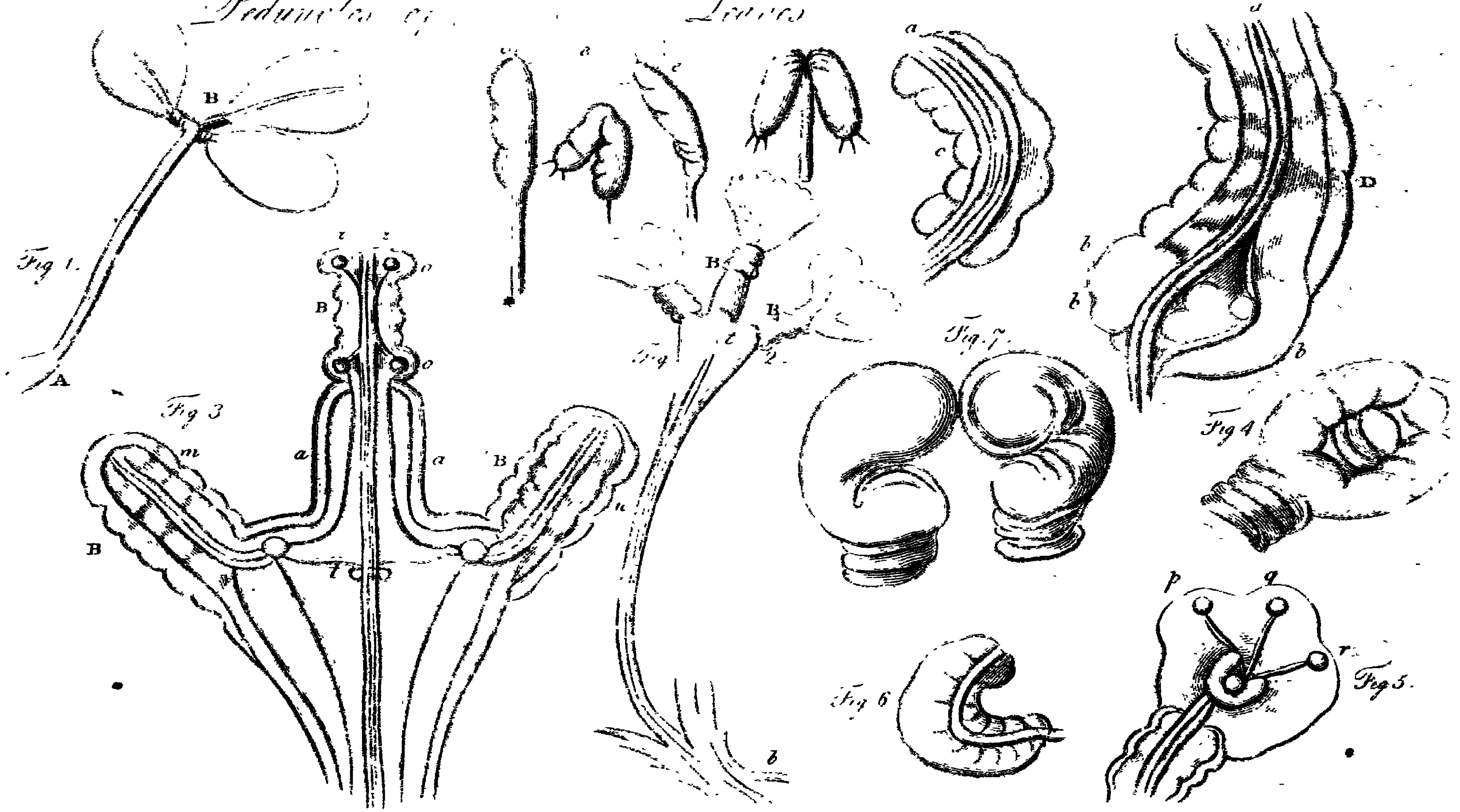
A strange mistake.

Fig. 2 is the plant: B B is the upper gatherer, but it has instead of the under one a stipula, which seems by some means (which I have not yet been able to comprehend) to serve instead. All the trifoliums and numbers of the diadelphian plants, have it thus. Fig. 3 shows this part dissected and explained. I have never found the balls *z z* except in the medicagoes, and not in all of these. There is not any thing more curious than the substance of which the balls are formed. It strongly resembles the matter of the bark without the inner bark vessels, is extremely watery, is the first part that decays, and appears to serve no other purpose, than to fix the string in its place. It is curious, that at *t*, where the knot comes, there is a fastening which passes entirely through the plant. The gatherers *m* and *n* at the side have no balls. There is another kind of a gatherer of a very curious form, which is found in the papilionaceous tribe. It has but one ball; but the same matter, being collected into a thick lump, is folded into creases (see fig. 4, and the dissection fig. 5, *p q*); and have a ball in a semicircular socket; it turns it to one crease, or the other, by means of the spiral wire. Fig. 5 better displays this, being a side view, and showing how it turns to the right or left, by taking the upper or lower crease, which of course turns the leaves nearly a whole circle. Fig. 6 shows the string when drawn tight in the gatherer. This will serve to prove the thorough mistake of those physiologists, who pretend, that the different parts of a plant may be changed for each other, and make a peduncle or leaf take root. Nature does not execute her work in this careless manner, each part has its separate mechanism, than can perform only the part assigned. If a flower bud is concealed in the peduncle, it may by accident grow, since the lower part of the gatherer, which joins the stem, is full of flower buds: but then it is these that grow, and not the leaf-stalk; nor can there be any thing more different, than the peduncle stem.

I shall give no farther examples this time, as what I have already said will be, I hope, sufficient to make what I have drawn understood, and to give some idea of the mechanical management of this part of most plants; accounting for the beautiful arrangement of the leaves of trees: and proving,

not

Peduncles of Leaves



not only that the spiral wire is the cause of motion in plants, but that the management of a plant is wholly mechanical.

I am, Sir,
Your obliged Servant,
AGNES IBBETSON.

I shall in my next give some account of the form of those sessile leaves, which belong to annuals, and those which are of the order pentandria digynia, as there are many curious particulars, which belong to both, and which I have not at present time to detail.

V.

On the Decomposition of Water in two or more separate Vessels. In a Letter from ADAM ANDERSON, *Esq.*

To W. NICHOLSON, Esq.

SIR,

THOUGH the detection of erroneous statements in matters of science is certainly a more humble task than the discovery or generalizement of facts, it must still be regarded as contributing, at least in some degree, to the progress of true knowledge, in so far as erroneous views have a tendency, not only to supersede experimental investigation, but to waste the energies of the mind in attempts to explain a state of things, which has no real existence in nature. I have been led to this remark, by reflecting on the difficulty, which chemists have hitherto experienced, to explain the transmission of the elements of water, during the decomposition of that fluid by galvanism, when a metallic wire forms part of the circuit, and the experiment is performed in separate receivers.

Detections of errours in science important.

Difficulty in explaining the galvanic decomposition of water in separate vessels.

I have ascertained, beyond the possibility of doubt, that the transmission of oxigen and hidrogen in opposite currents through the connecting wire is, contrary to the assertion of Ritter[*], entirely fallacious—that the supposition

Oxigen and hidrogen not transmitted through the wire in opposite currents.

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[*] Journal, 4to edition, vol. IV, p. 512.

of such a transmission must have arisen, either from an inaccurate mode of performing the experiment, or from a hasty and unwarranted generalizement of the repulsions and attractions supposed to be exerted at the opposite poles of the galvanic battery.

Decomposition of water in a single vessel,

Most of your readers are aware, that, when gold wires proceeding from each extremity of a moderately powerful galvanic battery, in a state of action, are introduced under a receiver filled with water, and inverted over a basin containing the same fluid, as at Pl. VI, fig. 1, the wire P being connected with the zinc side, and the wire N with the negative side, a decomposition of the water immediately ensues, oxigen is evolved at *p*, and hidrogen at *n*. The decomposition even goes on, when the wires are inserted in separate receivers, fig. 2; attended with this remarkable circumstance, that oxigen alone is found in one receiver, and hidrogen alone in the other. As we are forced in the present state of our knowledge, to believe, that a decomposition of the water takes place at the extremity of each wire, we must also admit, that the oxigen evolved at *n* is expelled by the negative, and attracted by the positive point, while the hidrogen evolved at *p* is repelled by the positive, and attracted by the negative point; so that, during the decomposition contrary currents of oxigen and hidrogen are proceeding along the dotted line *n a p*. Nay, we must even admit, that the force of these attractions and repulsions is sufficiently powerful, not only to separate the elements of water from a state of combination, but also to overcome the mechanical tendency to ascend through the water, which these elements possess in their gaseous condition.

and in one vessel with separate receivers.

Supposed repulsion and attraction of the oxigen and hidrogen,

sufficient to prevent their ascent through the water.

Similar phenomena said to take place when the water is in separate vessels.

All this may be admitted without much difficulty; but the fact stated by Ritter is by no means so easily explained; and indeed it has never been yet accounted for, without having recourse to the most improbable supposions. This philosopher affirms, that when the receivers *ab*, *cd*, fig. 3, filled with water, and inverted over separate vessels, A B, C D, are connected by a gold wire, *p n*, if the wires P, N, from the opposite extremities of the battery be immersed into the water contained in the vessels A B, C D, a decomposition

Decomposition of Water by Galvanism.

Fig. 1.

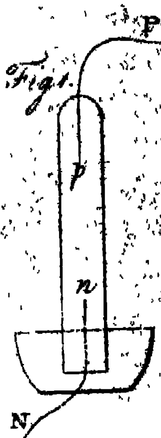


Fig. 2.

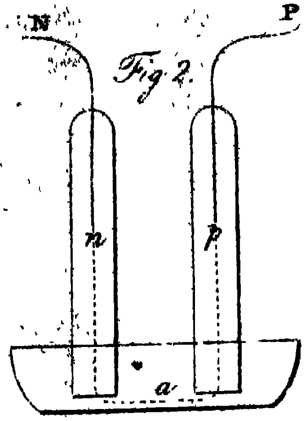


Fig. 3.

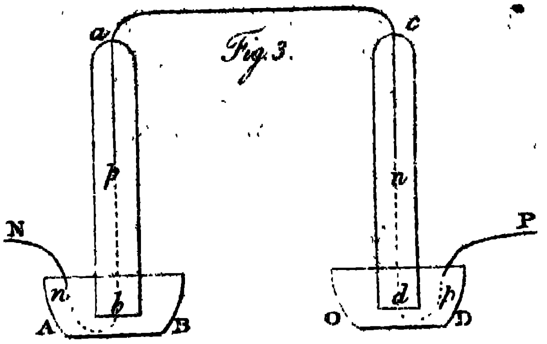


Fig. 4.

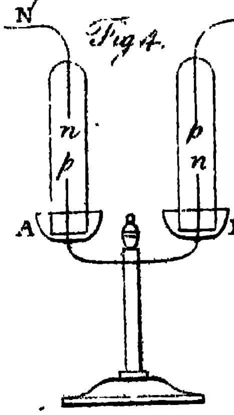


Fig. 5.

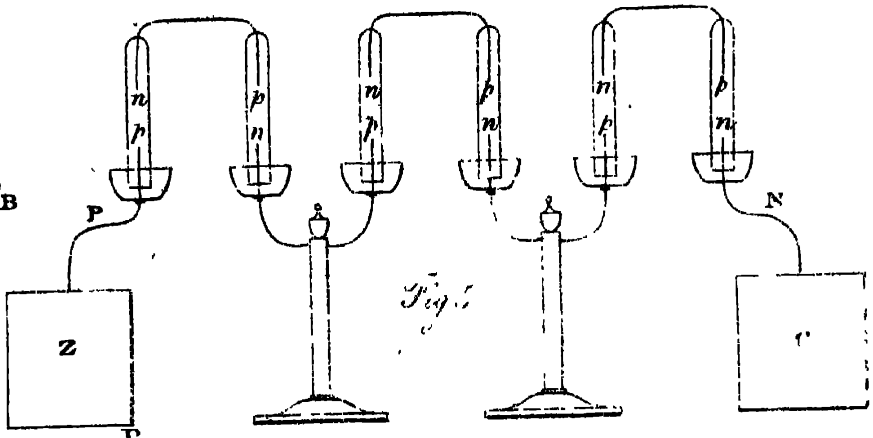
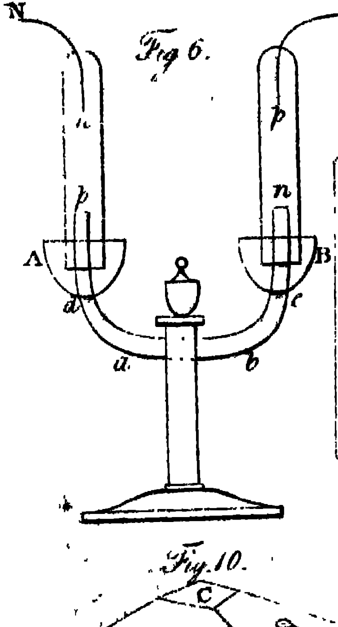


Fig. 6.



Carbonated Lime

Fig. 7.

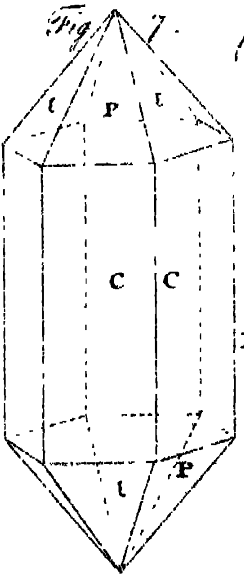


Fig. 9.

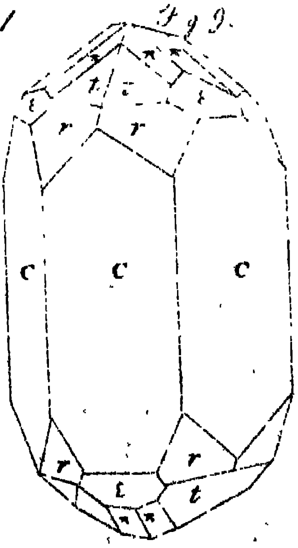


Fig. 8.

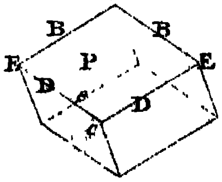


Fig. 10.

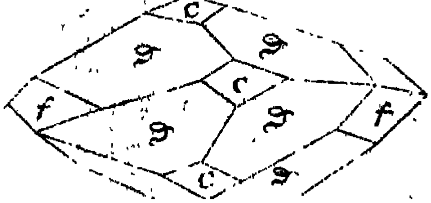
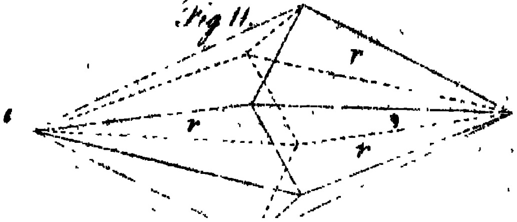


Fig. 11.



position of the water in the receivers takes place, accompanied by the same result as before, oxigen alone being found in one of the receivers, viz. *a b*, and hidrogen alone in the other, *c d*. Hence he concluded, that as a decomposition of the water must have taken place at each extremity of the connecting wire, the oxigen must have passed through that wire from *n* to *p*, where it was evolved, and the hidrogen in the contrary direction from *p* to *n*.

The gasses supposed to flow through the wires.

This explanation, so much at variance with all our notions of the impermeability of dense metallic substances by gaseous bodies, seems to have been reluctantly adopted by the greater number of chemists; while to a few it has appeared so inadmissible, that, rather than embrace it, they have been led to doubt the truth of the opinions commonly received with respect to the compound nature of water. No person, however, appears to have suspected the accuracy of Ritter's statement, or even to have repeated his experiments with any degree of care. The experiments, which I shall now describe, and which, I trust, will be deemed worthy of a place in your Journal, prove, in the most satisfactory manner, that the transmission of the elements of water in opposite currents through the connecting wire is altogether deceptive, and that the opinion of such a transmission taking place is founded on the want of a due attention to all the circumstances of the experiment.

The improbability of this has led to a doubt of the composition of water.

When I first repeated the experiment of Ritter, the result, I confess, appeared very singular; I saw no way of explaining why the oxigen and hidrogen were found separately, without adopting the opinion of Ritter, or denying that water was a compound of these two elementary substances. At length, however by reflecting more maturely on the subject, I began to suspect, that there might be a positive and a negative point in each receiver taken in conjunction with the corresponding cup, over which it was suspended: that the extremity of the wire P, fig. 3, connected with the zinc side of the battery, being positive, and the water acting as a conductor to the galvanic energy, the positive state would be conveyed through the water to the connecting wire *n p*, so that the extremity *p* would also become positive; while, for

The experiment repeated.

The phenomena explained by the positive and negative states taking place in each receiver.

for a similar reason, the opposite extremity *n* would become negative: that, consequently, as there was a positive and a negative point in the water connected with each receiver, it was obvious, that the decomposition would be effected by mutual attractions and repulsions subsisting between the elements of water, and the two contiguous points of the interrupted circuit, which were thus immersed in the same fluid; in short, that Ritter had been misled by overlooking the decompositions, which, I conceived, took place at the extremities *p* and *n* of the wires connected with the battery.

Experiments to prove this.

I accordingly adopted a new arrangement, as at fig. 4. I caused the wires proceeding from the battery to pass through the upper part of the receivers (which were hermetically sealed) and then placed the receivers over the connecting wire *p n*, supported on a stand, and passing through the two glass capsules A B. By this disposition of the wires connected with the battery, I was sure of collecting any gasses which might be evolved at their extremities. The result answered my expectation. I now obtained, not oxigen in the one receiver, and hidrogen in the other, but these two substances in each, in the exact proportion, in which they combine together to form water: for on passing the electric spark through the gasses collected in each receiver, separately, a detonation took place, the gasses entirely disappeared, and water was regenerated. The nature of the decomposition, which happened in each receiver, was obvious: the wire P, proceeding from the zinc side of the battery, being positive at the extremity *p*, and the water in the receiver operating as a conductor, the positively electric state was transmitted through the water to *n*, and then along the connecting wire *n p* to *p*, which by this means became also positive; in like manner, the wire *n* connected with the copper side of the battery, being negative at the extremity *n*, and the negatively electric state being transmitted through the water to *p*, and then along the connecting wire *p n* to the extremity *n*, this extremity became negative. There being thus a positive and a negative point in each receiver, the decompositions which took place differed in no respect from those which happen when the arrangement represented at fig. 1 is employed.

It

Every interruption of the circuit affords a positive and negative point.

It now occurred to me, that every interruption of the circuit would afford a positive and a negative point; and that a series of decompositions might be procured, by following out the same arrangements in a succession of receivers. I therefore constructed an apparatus first with four interruptions in the circuit, and afterwards another with six, fig. 5; and in both cases, I obtained in each receiver, the elements of water in the proper proportions, in which they combine to form this fluid. The positive and negative points are marked in order.

An experiment of Mr. Murray's repeated.

Though these experiments were perfectly decisive with regard to the effect produced by the connecting wire, and sufficiently calculated to unfold the real nature of the decomposition, to which it was subservient, I could not rest satisfied, till I had repeated an experiment, which Mr. Murray seems to adduce in confirmation of the imaginary transmission. I say, seems to adduce, for the experiment is stated with so little precision (considering the usual accuracy of this excellent chemist), that it is difficult to discover the real object, for which it is brought forward. After mentioning the experiment of Ritter, and adopting the conclusion which he deduced from it, he adds—“I have found, too, that if a portion of quicksilver be interposed between two portions of water, (which can be easily done by filling the bent part of a siphon with quicksilver, and putting water into each leg) on placing wires connected with a galvanic trough in the separate portions of water, gas arises from each wire[*]”. In order to repeat the experiment of Mr. Murray, I constructed an apparatus, such as I have represented at fig. 6. *p a b n* represents the bent siphon, the opposite ends being introduced through two glass capsules, A, B, to which they were hermetically sealed at the bottom, *d, e.* Having filled the capsules and the bent siphon with water, I inverted over the extremities of the siphon two small receivers filled with water, through the ends of which I had previously passed the gold wires N *n*, P *p*, and to which they were sealed by melting the glass. I then connected the wire N *n* with the upper side of the battery, and P *p*

Decomposition of water in separate vessels with quicksilver interposed.

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[*] Murray's Chemistry, vol. I, p. 558.

Oxigen in one vessel and hidrogen in the other, when water only was employed:

with the zinc side, and immediately gas was disengaged at their respective extremities *n* and *p*. On examining the gasses obtained in the two receivers, the gas in the receiver connected with the negative side of the battery was hidrogen, and that in the receiver connected with the positive side, oxigen. This arrangement did not differ essentially from that represented at fig. 2; and the reason why the gasses are found separate is equally applicable to both.

but when mercury was interposed,

this was oxided in one receiver, and left pure hidrogen, while the other receiver contained both oxigen and hidrogen.

I then removed the water out of the bent siphon, and supplied its place with mercury, confidently expecting, that the mercury (making allowance for its oxidable property) would operate precisely as the connecting wire in the arrangement represented at fig. 4. Accordingly, on connecting the wires N and P with the opposite sides of the battery, in a few seconds I perceived an oxidation of the mercury taking place at the point *p* of the bent siphon, which, as the wire P *p* was connected with the zinc side of the battery, was a positive point. Gas was copiously disengaged at the opposite extremity of the siphon, as well as from the points *n* and *p* of the connecting wires. After allowing the decomposition to go on during some minutes, I examined the gasses in the two receivers. The gas in the receiver over the capsule B exploded by the electric spark, and disappeared completely, while no effect whatever was produced by passing a succession of electric sparks through the gas in the receiver over A. I therefore introduced into this receiver as much oxigen, by measure, as was equal to half the bulk of the gas which it already contained, and which I had no doubt was pure hidrogen: I then passed the electric spark through the mixture, when an explosion took place, and both gasses completely disappeared.

Ritter therefore misled.

This experiment, therefore, so far from supporting the opinion of Ritter, shows, that he must have been misled by a partial view of the circumstances attending the decompositions, while it affords an additional illustration of what I have already stated with respect to a series of alternately positive and negative points at every interruption of the circuit.

Another experiment with an

Pursuing still farther the idea of this alternation of the electric states, I cemented to a glass rod a succession of small bits of gold wire, and having interposed them in that state, between the extremities *p* and *n*, fig. 1, of the two wires connected with the positive and negative sides of the battery, I observed, with pleasure, a considerable disengagement of gas taking place, at the same time, from each extremity of all the unconnected wires, which formed the galvanic circuit.

Interrupted circuit.

Having thus pointed out the circumstances which misled Ritter and his followers, and established, beyond all doubt, the important fact of a positive and a negative point at every interruption of the circuit, it is almost unnecessary to observe, that the decompositions, which happen by employing the arrangement first suggested by that philosopher, admit of being explained on the same principles as the decomposition effected by introducing under the same receiver a positive and a negative point, proceeding immediately from the galvanic battery.

The principle of the decomposition therefore the same in all cases.

I am, Sir, your most obedient servant,
ADAM ANDERSON.

Perth Academy,
Sept. 23, 1811.

VI.

Description of several new Varieties of carbonated Lime: by Mr. Hauy*.

THOSE problems, of which the object is to determine the varieties of a crystallization having a rhomboid for its primitive form, are susceptible of two solutions, which lead to the same figure by different laws of decrement. Mechanical division, by making known the position of the faces of the nucleus with respect to those of the secondary crystal, shows on which of these two laws the figure of a given crystal depends. In the course of a long time I had very seldom met with the two solutions at once in the same system of crystallization; but instances of this kind have been more numerous among the recent observations I have made

Two laws of decrement for a rhomboidal nucleus, determinable by cleavage.

Sometimes nature follows both.

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* Journal des Mines, vol. XXIII, p. 49.

on the varieties of carbonate of lime, of which I have now 93 in my collection. I shall describe some of these, which realize the possibility of this double employ of the same figure with two different structures.

Trihexaedral carbonate of lime.

The trihexaedral carbonated lime, Pl. VI, fig. 7*, a specimen of which was presented me by Mr. Hericart de Thury, exhibits itself in the form of a regular hexaedral prism C C′, terminated by two right hexaedral pyramids P ε. Three faces, P, of each pyramid, taken alternately, are parallel to those of the nucleus. The other three, designated by ε, which arise from a decrement by two rows in height on the lower angles of the nucleus, are inclined to the adjacent sides at the same angle as the preceding, namely 135°; so that the secondary rhomboid, which the union of these faces would produce if they existed alone, would be similar to the nucleus.

This result, which I have demonstrated in the geometrical part of my treatise, may be considered as the limit of all those, to which the double solutions I have spoken of lead; because it is that, in which, one of the two quantities expressing the decrement becoming 0, the solid answering to this term is the nucleus itself.

Ambiguous carbonate of lime.

In the ambiguous carbonated lime, fig. 10, the dodecaedron $\vartheta\ \vartheta$, which in this variety is combined with the inverse rhomboid *f f*, and the sides C C′ of the regular hexaedral prism, is similar to the metastatic dodecaedron, vulgarly dogtooth spar; but it depends on a different law of decrement, of the kind of those I have called intermediate. This result requires a certain explanation to be well understood.

Common metastatic dodecaedron.

In the common metastatic dodecaedron, fig. 11, the least saliant edges answer to the faces of the nucleus, while the most saliant are turned toward its edges. I had inquired, when I wrote the geometrical part of my treatise, whether there were not a law of decrement capable of producing a secondary crystal similar to the metastatic, so that the edges turned toward the faces of the nucleus should be, contrary to it, the most saliant; and I found, that this result would take place from the intermediate decrement ${}^{\frac{5}{4}}E\ {}^{\frac{5}{4}}B\ {}^{1}D^{2}$.

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* Fig. 8 represents the primitive form.

On

Common inverse rhomboid.

On the other hand, the common inverse rhomboid has its faces turned toward the superior edges of the nucleus: and, having also examined what law would give the same rhomboid, with its faces answering to those of the nucleus, I was led by calculation to the result expressed by $\overset{5}{e}$.

Combination of these.

Structure of the present variety.

Let us suppose, that the common inverse rhomboid is combined in one figure with the common metastatic dodecaedron; it is evident, that its faces would answer to the most saliant edges of this dodecaedron: but in the variety before us, on the contrary, they answer to the least saliant edges. Now there are two different cases, in which this may take place: one is that in which the metastatic would result from the law $\overset{2}{D}$, and the inverse rhomboid from the law $\overset{5}{e}$; the other, that in which the metastatic would be produced by the intermediate decrement, and the rhomboid by the decrement $E^1\,{}^1E$. Mechanical division removes all ambiguity by proving, that the second is the case. The faces of the two solids combine, as I have said, with the sides of the hexaedral prism, from which we can derive no indication in favour of one structure, or of the other.

Stenonome carbonate of lime.

The stenonome carbonated lime, fig. 9, differs from that which I have described in my treatise under the name of subtractive by the addition of the facets s and π. The former afford a fresh example of the law of decrement, which tends to produce a rhomboid similar to the nucleus. The faces π π exhibit a particular case, the possibility of which I had proved; namely that in which the decrement on B, fig. 8, taking place by two rows, would produce a dodecaedron, all the triangles of which, instead of being scalene as in the other cases, would become isosceles; that is to say, the dodecaedron would be composed of two right pyramids united base to base. In fact we should have a dodecaedron of this kind by prolonging the faces in question till all the others had disappeared.

Proportions between the angles,

The angle of 151° 2′ 42″, which measures the respective incidences of the faces of this dodecaedron, is exactly double the angle of smallest incidence of the faces of the nucleus, 75° 31′ 21″. These proportions between the angles of the primitive form and those of the secondary crystals are not unfrequent in the varieties of carbonated lime.

Crystals formerly supposed.

From these examples it is seen, that results, which I had given as merely hypothetical, appear as descriptions by anticipation of so many products of crystallization, which existed in the bosom of the Earth without our knowledge.

VII.

Extract of a Letter from Dr. FRANCIS DELAROCHE *to* F. BERGER, *Esq.*; *on Radiant Heat and other Subjects. Communicated by the latter Gentleman.*

PARIS, *July the* 17*th*, 1811.

Phenomena of radiant heat.

IN my last two letters, I mentioned to you an inquiry into the phenomena of radiant caloric, which I commenced last spring, and of which the principal results are the following.

Radiant caloric, almost entirely divested of the faculty of traversing glass, when the substance that emits it is at less than 100° [212° F.] or even 180° [356° F.], acquires this property very manifestly, and independent of the light that may accompany it, in proportion as the temperature of the heated body is increased beyond this.

The rays emitted simultaneously by one and the same heated body differ from each other with respect to the faculty of traversing glass.

The quantity of radiant caloric emitted, or, to speak more properly, the quantity of caloric arriving at a distance in the radiant form is not proportional to the temperature of the heated body, as commonly supposed, but it is infinitely greater in proportion at high temperatures, than at lower.

Lastly, that the law of refrigeration established by Newton, though nearly accurate at low temperatures, is far from being so at high ones.

Phenomena of light.

Nothing very striking has occurred here in the sciences within these few months. Mr. Malus is still pursuing with success his inquiries concerning *polarised* light. Mr. Arago likewise is making some curious experiments on the same subject. Some, that he has lately made on the illumination

Illumination of the sun.

mination of different parts of the solar disk, show, that the degree of illumination of the edges and of the centre is precisely the same, contrary to the opinion generally received. Mr Clement has very happily applied prof. Leslie's process for the formation of ice to the rapid and complete dessiccation of various animal and vegetable substances. He has also greatly improved the apparatus for evaporating liquids by the help of fire.

Dessiccation of animal and vegetable substances

Evaporation.

VIII.

On Chemical Attraction. By MARSHALL HALL, *Esq.*

To W. NICHOLSON, Esq.

SIR,

CHEMICAL attraction is that force, by which the particles of matter are drawn towards each other. These particles are of two kinds; for they may be similar to each other, as in the same simple body, when they are termed homogeneous; or they may be dissimilar, as in a compound body, and are then denominated heterogeneous. From this distinction between the particles of material objects, a division of the attraction, which unites them, immediately flows. The force, which occasions similar particles to cohere, is called homogeneous attraction; dissimilar particles are united by heterogeneous attraction: the former is the cause of cohesion in *simple* bodies; the latter occasions combination between different bodies.

Chemical attraction.

Homogeneous and heterogeneous attraction.

But, beside these, philosophers have supposed, that a third order of particles, and of attraction, influences chemical actions. " Heterogeneous affinity urges heterogene-" ous particles toward each other, and of course is the cause " of the formation of *new integrant particles*, composed of " a certain number of heterogeneous particles. These new " particles afterward unite by cohesion, and form masses of " compound bodies*." In the words of Mr. Murray, " the " integrant particles are merely the smallest particles, into

A third order of particles and of attraction supposed.

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* Thomson, ed. 3, vol. III, p. 408.

"which a substance can be resolved without decomposition. "The integral parts are united by the force of aggregation, "the constituent parts by chemical affinity*." Berthollet describes the force of cohesion of a compound, as that by which the integral parts are held together†.

This has led to errours in the general theory of chemical attraction.

It is the object of the following observations, to point out what I conceive to be an inaccuracy, in the opinion of compound integrant particles, and of the attraction by which they are supposed to be united; and especially to notice some errours, which have been introduced into the general theory of chemical attraction, by the adoption of this opinion.

Opinion of compound integrant particles hypothetical.

It is proper to premise, that the opinion itself of compound integral particles must be admitted to be hypothetical. We mix two substances together, and their particles unite in that manner, which constitutes chemical combination; but to say in what precise manner they unite, I apprehend to be impossible; that they first collect together to form particles of a new kind, and of a superior order, which unite by homogeneous attraction, is surely not very manifest. It is perhaps more probable, that chemical union is a less complicated operation. If a number of heterogeneous particles be mixed together, they assume respectively that situation, which their mutual attraction allots to them; every particle is probably attracted by every other; and of this attraction, combination and aggregation are equally the effects.

Combination and aggregation effects of the same cause

Nor can the cohesion of a compound substance be attributed more to the agency of homogeneous, than of heterogeneous attraction; for if, in a compound, the particles be drawn towards each other, it is of no importance whether these particles be similar or dissimilar; the same effect, in point of cohesion, will be produced.

The contrary hypothesis improbable.

The account therefore usually given of the formation of the integrant particles of a compound, which unite by homogeneous attraction, or cohesion, is not only without proof, but, as I humbly conceive, without probability. We shall however admit the opinion, and proceed to consider how it accords with and explains the phenomena of chemical combination.

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* Murray, ed. 2, vol. I, p. 63. † Researches, p. 38.

Cohesion supposed to modify the action of chemical affinity.

Berthollet, in his researches on this subject, has ascribed many phenomena to the operation of the homogeneous attraction, which unites integrant particles, or, as it is termed by him, cohesion. He considers it as a powerful cause in modifying combination; and especially, he attributes many of the results of complex affinity to its influence; he supposes, that Bergman's Tables do not represent the real order of the affinities of bodies, but rather, the degree of cohesion possessed by the compound when formed[*].

Instanced in sulphate of potash and muriate of lime.

The following illustration is given of the mode of operation of this force of cohesion. "If a solution of sulphate "of potash be mixed with muriate of lime dissolved in a "small quantity of water, the lime brought into contact "with the sulphuric acid will be more powerfully influenced "by the force of cohesion, than the potash. It is therefore "a force in addition to those which preexisted, and deter"mines the combination of the sulphuric acid with the lime, "and the precipitation of the new compound[†]."

This instance examined.

As this paragraph comprehends much of the doctrine of the influence of cohesion in modifying chemical union, it deserves particular notice, and it will be of advantage to make a few observations on it.

The lime does not exert a stronger force of cohesion than the potash before decomposition has taken place.

It may be inquired, what is to be understood by the lime being brought into contact with the sulphuric acid? Is chemical contact or chemical union intended? It is difficult to determine this question. If chemical union be not intended by the word contact, it is improper to say, that the lime will be more powerfully influenced by the force of cohesion, than the potash; for muriate of lime is more soluble than sulphate of potash. Let us suppose, that chemical union is intended, and we shall still observe a manifest impropriety in the account of the influence of cohesion which follows.

A power that does not act till an effect has taken place cannot have produced the effect.

It is said, that cohesion is a force, in addition to those which preexisted, and determines the combination of the sulphuric acid and the lime, and the precipitation of the new compound. Now it is to be observed, that this new force can only be exerted, when "the lime is brought into

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[*] See Researches, p. 106.

[†] Ibid, p. 105.

" contact with the sulphuric acid;" how then does it afterward " determine the combination of the sulphuric acid " with the lime?" A power which is only evolved at the instant of the combination of two substances, cannot surely influence, or *determine* that combination.

Cohesion is a power, which is exerted between *integrant* particles only; in this instance between the integrant particles of sulphate of lime; it has no influence before their existence, and consequently cannot contribute to their formation, it cannot therefore be a power in addition to those which preexisted, so as in its operation to determine the combination of the sulphuric acid and the lime.

It appears to me, therefore, that Berthollet has attributed the formation of saline compounds to the active energy of a power, the very existence of which, according to his own definition, must be coeval with, and cannot precede and influence their formation.

Cohesion between the particles of a compound is a force that determines its formation.

Now it is to be observed, that the proposition, that cohesion in a compound is a force which determines the formation of that compound, is really a fact as well established as any in chemistry. For, " if all the decompositions ascribed " to complex affinities be investigated, it will be found, that " the prevailing affinity has been always ascribed to those " substances, which have the property of precipitating, and " of forming a salt, which can be separated by crystal" lization *." The formation of these compounds, therefore, can scarcely be attributed to any other cause, than that which Berthollet alleges; namely, the operation of the attraction of cohesion in the compounds formed.

Combination and aggregation the joint effect of heterogeneous and homogeneous attraction.

On the contrary, that cohesion is exerted between compound integrant particles only, nay, the very existence of such particles, is entirely hypothetical. The former proposition is supported by an ample number of experiments: the latter, which is in contradiction to it, is merely matter of opinion. Experiment, which is the light of Nature, shows us, that that power, which we term the attraction of cohesion, does influence and determine the combination of those substances, or of those particles, which constitute a compound with much cohesion: but, as it has been shown, these particles cannot be what are termed *integrant*; the constituent particles of a compound are therefore made to approach by the agency of the force of cohesion, just in the same manner as by chemical attraction. Where then is the distinction between these two powers? To me it appears, that there is no distinction whatever; but that, in fact, aggregation and combination are both the effects of the mutual attraction of heterogeneous particles. In the compound A B, each particle of A is probably attracted by every other particle of A, and by every particle of B. Now the first of these attractions is homogeneous; the second heterogeneous. It is therefore probable, that the particles of every compound unite and adhere by the agency of both these kinds of attraction; it is surely improper to assert, that they *unite* by the agency of one attraction, but *adhere* by the influence of the other.

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* Berthollet, Researches, p. 106.

It is proper to observe, that the change, which is here suggested with regard to our opinions of the attractions of cohesion and of combination, is not so singular as at first view it may be supposed to be. A change precisely analogous has been proposed, relative to the operation of affinity between two or more compounds. Formerly it was supposed, that, when two binary compounds, for example, are submitted to mutual action, the energy exerted in their union subsisted between the integrant or homogeneous particles of these compounds. A view of the subject, very different, has however been given by Berthollet. He supposes, that two compounds act on each other by an affinity *resulting* from the united energies of their constituent elementary particles. A change precisely similar is here suggested relative to the attraction of aggregation in a compound body. The prevailing opinion is, as it was formerly with respect to chemical affinity, that the attraction is exerted between compound particles. I suggest, that, as in affinity, the powers may be exerted between constituent and elementary particles. Both powers may with equal propriety be termed *resulting* attractions.

Consequences of this opinion.

If this opinion concerning chemical attraction be correct, certain consequences will necessarily follow, which it may be proper to point out. 1st. Those compounds, the constituents of which have the greatest affinity, will have the most cohesion; they will be the least soluble, and the first to crystallize on evaporation. 2dly. Bergman's Tables may still be considered as representing, in some degree at least, the affinities of bodies. 3dly. Berthollet's opinion, that, when two binary compounds are dissolved together, a quaternary compound is always formed by their union, will be in great measure invalidated. These consequences we shall endeavour to trace; so that, if the opinion we have stated be just, it may receive due confirmation, from the observation of the phenomena presented by chemical attraction.

Compounds that have the greatest affinity least soluble.

The first proposition is abundantly proved by the following observations of Berthollet, which deserve to be quoted a second time. "If all the decompositions ascribed to complex affinities be investigated, it will be found, that the "prevailing affinity has been always ascribed to those substances, which have the property of precipitating, or of "forming a salt, which can be separated by crystallization. "For this reason it may be inferred *a priori*, from a knowledge of the solubility of salts which may be formed in a "liquid, that those substances, which are least soluble, and "most apt therefore to precipitate, will be found to be the "same as those to which Bergman and other learned chemists have attributed the strongest affinity in their tables," &c.—See Researches, p. 106 et seq.

Sulphates of barytes and potash.

Barytes has a stronger affinity for sulphuric acid than any other base; it therefore decomposes all the sulphates. From the same energetic attraction the particles of sulphate of barytes cohere with more force, and it is found to be less soluble than the other sulphates. Thus we consider the forcible attraction, which subsists between sulphuric acid and barytes, as at once the cause of the decomposition of the sulphate of potash, and of the strong cohesion, and of the little solubility of the new sulphate. This account of the matter I think is perfectly just and reasonable, whereas we have shown the incongruity of the opposite opinion.

Bergman's Tables represent the real affinities of bodies.

We are now naturally led to consider our second proposition; that Bergman's Tables may still be considered as representing the real affinities of bodies. If Berthollet's opinion, that the decomposition and separation of salts arise from the influence of a power altogether distinct from chemical affinity, be correct, it is obvious, that Bergman's Tables merely represent the cases, in which this power is most energetic, and do not denote the order of affinity of the various substances comprehended in them: but if, on the contrary, it be proper to consider the force of cohesion in a compound as depending in equal measure on heterogeneous and homogeneous attraction, then it as obviously follows, that Bergman's Tables do indeed represent the order of affinities of the substances arranged in them.

It will be very evident, that this question is of great importance; and I hope what has been said, together with some observations connected with our third proposition, will render the view we have taken of it sufficiently probable. It is to be observed, that Bergman's Tables of Chemical Affinities may err from other causes; sufficient regard may not have been paid to the proportions, volatility, &c. of the substances; it is only in reference to the supposed influence of the attraction of aggregation of the compound, that they may still be regarded as expressing real affinities. The force of cohesion of the individual constituents of a compound will influence the formation and the state of aggregation of that compound; for I suppose, that the force, which causes the particles of a simple body to approach each other, is not destroyed or suspended, when that body enters into combination. Yet still, as this degree of cohesion between the particles of any body may be considered as a property of that body, Bergman's Tables may be considered as denoting the order of affinity in any number of bodies endowed with such properties.

Two binary compounds may exist together in solution, not forming a quaternary compound.

Our third proposition has been discussed with much ability by Mr. Murray, who considers it as equally probable *a priori*, that two binary compounds should exist together in solution, as that they should unite to form a quaternary one. He adds, "it is very doubtful, whether Berthollet 'has not extended too far the principle, on which his theory 'of complex affinity is established."

Or why is not the quaternary compound obtained by evaporation?

If a quaternary compound be formed on the solution of two binary ones in water, it is a natural question, why is not this quaternary compound obtained by evaporation? Here the influence of the attraction of aggregation is alleged as the cause of the decomposition of the quaternary compound, and of the formation of the two binary compounds obtained by crystallization; but in this instance the same inconsistency occurs, as has been pointed out, in considering the formation of sulphate of lime on the same principle. A power developed at the instant of the formation of a compound is represented as the *cause* of the formation of that compound.

Hence it appears, that, supposing the solution to contain a quaternary compound, no reason can be given, why this compound is not obtained, on the evaporation of the solvent; the inference therefore must be, that such a compound did not exist in solution; but that the substances dissolved are really binary compounds, such as we obtain them.

And even on the opposite supposition, that cohesion does not at all depend on heterogeneous affinity, I think the same inference might be deduced. If a solution contain a quaternary compound, which becomes two binary compounds on evaporation; this change must take place, at the instant of crystallization: but why it does take place at that instant, we are not told; there is no new power called into action; because Berthollet's notion of the force of cohesion is, that it is a power not only when apparently effective, but also when it appears to be entirely overcome. The question then remains, if the solution be that of a quaternary compound, what is the reason, that, at the point of crystallization it is decomposed, and that two binary compounds are obtained on evaporation?

Influence of volatility.

Speaking of the influence of volatility on affinity, Berthollet remarks, that "heat, by increasing the volatility of "a substance, enfeebles its combination, and this cause is not "less efficient in complex, than in elective affinities: it is a "force added to those already in action, and which determines "the union and separation of *those substances*, which are most "disposed to form a *volatile compound*." Still the same objection may be urged against this observation, as was offered to the alleged influence of cohesion in effecting combination. The volatility of a compound can have no influence on the formation of that compound; combination, in every instance, depends on the properties of the constituents, and

That of a resulting compound cannot influence its formation.

not on the substance constituted. If the constituent parts of a compound be volatile, this volatility will be increased by heat, and their union will probably be promoted; but it is of no importance whether the compound be volatile or fixed when formed.

Causes of limits to chemical combination according to Berthollet.

Berthollet has explained in a far more satisfactory manner than former chemists the causes of some of the limits, which are observed in chemical combination; he has pointed out the influence of quantity, cohesion, volatility. But there still remains considerable difficulty in accounting for these limits on many occasions; as in the following instance, where condensation is considered as the cause, determining the proportion of the constituent parts of the compound, and affording the limit to combination.

Great condensation.

"When, in the *progress* of combination, the result in any "part of it is great condensation; this, by the obstacle it "may oppose to the exertion of affinity, or even by the "greatness of the condensation withdrawing the product "from the sphere of action, may limit the combination to "that point, or to the proportion at which this effect is "greatest; or if by particular circumstances this is over-"come, in the farther *progress* of combination it may hap-"pen; and in this way, compounds in two or three deter-"minate proportions may be formed.*" I am not certain, with what propriety we speak of the "progress of combination"; because I do not know, that we have any reason to believe it to be *progressive*; progressive in the sense implied, in relation to proportions. If oxigen be combined with hidrogen, the compound is established in a certain determinate proportion; nor have we any reason for supposing, that the combination ever took place in any other proportion; much leſs can we presume, that the proportion of one or the other increases progressively, until the occurrence of a considerable condensation puts a period to the progress of combination, and determines the proportions of the compound. The cause of the condensation is likewise unknown; unleſs it be attributed to the formation of new integrant particles, possessed of a new attraction; which is

Combination probably not progressive in respect to proportions.

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* Murray, vol. 1. p. 105.

Mr. Dalton's views of proportion.

an hypothesis, that cannot be admitted. On the subject of proportion, Mr. Dalton's views give the happiest explanation; but they must be acknowledged to be hypothetical. The particles of bodies are objects of science, and not of sense; yet we speak of them, as if they were as palpable and visible as the masses they compose.

Union of gasses on the application of heat, and by compression.

I shall conclude these observations by an allusion to two instances of combination, which have not, I think, been very satisfactorily explained. The first is the union of gasses on the application of heat; the second, the same combination by means of compression. They are both explained on the mechanical principle of an approximation of the particles, occasioned by the compression; but it is to be remembered, that, as both operations may be attended by compression, they are also both attended by an increased temperature. May not the heat have some more immediate operation in attracting these combinations, than is supposed: I believe the pressure, which causes the gasses to combine, requires to be applied very suddenly, so that a due quantity of heat may be evolved. It is requisite, that the application of heat to inflame gasses be applied suddenly, so as to ensure a considerable compression? The determination of both these questions by experiment would be extremely interesting. The influence of compression is seldom or never resorted to in pneumatic experiments; although it would undoubtedly prove an agent of considerable power in promoting combination. I am, Sir,

Yours respectfully,

MARSHALL HALL.

University, Edinburgh,
September the 25th, 1811.

IX.

On the Horticultural Management of the Sweet or Spanish Chestnut-tree. By the Right Hon. Sir Joseph Banks, *Bart, K. B. &c.*[*]

Chestnuts grafted on the continent,

IN all the northern parts of Europe, where chestnuts are used for food, the practice of grafting the trees that bear them has been known from time immemorial; the wild or ungrafted chestnut is called in french châtaignier, the grafted or cultivated sort maronnier.

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[*] Trans. of the Horticultural Society, vol. I, p. 140.

and in the west of England.

Though the grafting of chestnuts has been little, if at all used in this part of the island, it is not an uncommon practice in Devonshire, and other western counties. The nurserymen there deal in grafted chestnut trees, and the gentlemen have no doubt introduced them into their gardens.

About sixteen years ago, sir William Watson sent some of these grafted trees from Devonshire to Spring Grove, with an assurance, that the fruit would be plentiful and good. They were at first neglected and ill treated, owing to the disinclination most gardeners have to the introduction of novelties, the management of which they are unacquainted with: it was therefore six or seven years before they began to bear fruit.

The English much sweeter than the Spanish.

Since that time, as the trees have increased in size, the crop has every year become more abundant; last autumn the produce, though they are only six in number, was sufficient to afford the family a daily supply from the beginning of November till after Christmas. The nuts are much smaller than the Spanish imported fruit, but they are beyond comparison sweeter to the taste. The crops are little subject to injury, except from very late frosts. The trees are in general covered with blossoms to a degree, that retards their annual increase. They are now so low, that a part of the crop is gathered from the ground, and the remainder by a step-ladder. They require no care or attendance on the part of the gardener, except only the labour of gathering the fruit. Most people prefer the taste of the fruit to that of the imported, but there can be no doubt, that, when the usage of grafting chestnuts becomes common in this country, grafts of all other sorts will in due time be procured from the continent.

Mode of keeping them.

The kernels of these chestnuts, and of all others ripened in England, are more liable to shrivel and dry up than those imported, owing to a deficiency of summer heat in our climate to mature the fruit; this must be guarded against by keeping the nuts always in a cool place, rather damp than dry; the vessel best suited to preserve them is an earthen ware jar with a cover, this will not only keep them cool, but it will restrain the loss of moisture without entirely preventing perspiration, and thus endangering the loss of vitality, the immediate consequence of which is the appearance of must and mouldiness.

X.

On Potatoes. By Thomas Andrew Knight, *Esq. F. R S. &c*[*].

Suggestions with regard to the culture of potatoes

have succeeded in practice.

IN the Horticultural Transactions of 1807[†], I have described a method of cultivating early varieties of the potato, by which any of those, which do not usually blossom, may be made to produce seeds, and thus afford the means of obtaining many other early varieties. I also offered a conjecture, that varieties of moderately early habits, and luxuriant growth, might be formed, which would be found well adapted to field-culture, and be ready to be taken from the soil in the end of August, or the beginning of September, so that the farmer might be allowed ample time to prepare the same ground for a crop of wheat. I am now enabled to state, that the success of the experiment has in both cases fully answered every expectation that I had formed.

The tubers and blossoms produced by the same sap.

Large crops from varieties producing no blossoms.

The facts that I have stated in the Horticultural Transactions of 1807, and more fully in the Philosophical Transactions, are, I believe, sufficient to prove, that the same fluid, or sap, gives existence alike to the tuber, and the blossom and seeds, and that whenever a plant of the potato affords either seeds or blossoms, a diminution of the crop of tubers, or an increased expenditure of the richness of the soil, must necessarily take place. It has also been proved by others, as well as myself, that the crop of tubers is increased by destroying the fruit-stalks and immature blossoms as soon as they appear; and I therefore conceived, that considerable advantages would arise, if varieties of sufficiently luxuriant growth, and large produce, for general culture, could be formed, which would never produce blossoms.

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[*] Trans. of the Hort. Soc. vol. I, p. 187.

[†] See Journal, vol. XIX, p. 97.

Peculiar advantages of improved varieties of plants.

I have since had the gratification to find, that such are readily obtained, by the means which I have detailed, and I am disposed to annex more importance to the improvement of our most useful plants, than any writer on agriculture has hitherto done; because whatever increased value is thus added to the produce of the soil is obtained without any increased expense or labour, and therefore is just so much added to individual and national wealth.

Potatoes that ripen early in autumn do not necessarily vegetate early in spring:

I formerly supposed that all varieties of the potato, which ripened early in the autumn, would necessarily vegetate early in the ensuing spring, and could therefore be fit for use only during winter; but I have found that the habit of acquiring maturity early in the autumn is by no means necessarily connected with the habit of vegetating early in the spring; and therefore by a proper selection of varieties, the season of planting crops, for all purposes, may be extended from the beginning of March, nearly to the middle of May, and each variety be committed to the soil exactly at the most advantageous period.

but most produce to be expected from those that vegetate early.

A variety, however, which does not vegetate till late in the spring, and which ripens early in the autumn, cannot, I conclude, particularly in dry soils and seasons, afford so large a produce as one which vegetates more early: I, nevertheless, obtained so large a crop from one which vegetates remarkably late in the spring, and ripens rather early in the autumn, that I was induced to ascertain, by weighing, to what the produce would have amounted, had the crop extended over an acre, and I found, that it would have exceeded 21 tuns, 11 cwt. 80 lb*.

Produce.

In this calculation the external rows, which derived superior advantage from air and light, were excluded; and no more manure, or culture, than is usually given, had been employed; for the crop was not planted with any intention of having it weighed: the wet summer was, however, very favourable.

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* 48352 lbs.

Calculation of the proportion of food obtained from ground in potatoes,

I am not acquainted with the common amount of the weight of a good crop of potatoes, upon an acre of ground in a favourable soil, when well manured and cultivated; but I am confident, that it may generally be made to exceed 20 tons, by a proper selection of varieties: and if four pounds of good potatoes afford, as is generally supposed, at least as much nutriment as one pound of wheat, the produce of an acre of potatoes, such as I have described, is capable of supporting as large a population as eight acres of wheat; admitting the calculation of Mr. Arthur Young, that the average produce of an acre of wheat is 22½ bushels *: and as an acre of wheat will certainly support as large a number of people as five acres of permanent pasture, it follows that an acre of potatoes affords as much food for mankind as forty acres of permanent pasture: an important subject for consideration, in a country where provisions are scarce and dear, and where so high bounties on pasture are paid in the form of taxes on tillage, that the extent of permanent pasture is certainly and consequently increasing; and it must increase, under existing circumstances; for it pays a higher rent to the landlord, and relieves the farmer from much labour, anxiety, and vexation.

wheat,

and pasture.

Prevention of blossom would pay the rent of the ground.

To what extent a crop of potatoes will generally be increased by the total prevention of all disposition to blossom, the soil and variety being, in all other respects, the same, it is difficult to conjecture; but I imagine, that the expenditure of sap in the production of fruit stalks and blossoms alone would be sufficient to occasion an addition, of at least an ounce, to the weight of the tubers of each plant; and if each square yard were to contain eight plants, as in the crop I have mentioned, the increased produce of an acre would considerably exceed a tun, and of course be sufficient, in almost all cases, to pay the rent of the ground.

Varieties suited to Ireland not so to England.

I do not know how far other parts of England are well supplied with good varieties of potatoes; but those cultivated in this part of the island are generally very bad. Many of them have been introduced from Ireland, and to that climate they are probably well adapted; for the Irish planter is secure from frost from the end of April nearly to the end

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* 1440 lbs.

of November: but in England the potato is never safe from frost till near the end of May; indeed I have seen the leaves and stems of a crop, in a very low situation, completely destroyed as late as the 13th of June, and they are generally injured before the middle, and sometimes in the firſt week of September.

The Irish varieties, being excessively late, are almost always killed by the frost while in full blossom; when, omitting all consideration of the useless expenditure of manure, it may justly be questioned whether the tubers of such plants, being immature, can afford as nutritive, or as wholesome food, as others which have acquired a state of perfect maturity.

Importance of obtaining varieties suited to this country.

The preceding statement will I trust point out to the Horticultural Society the importance of obtaining improved varieties of the potato, and I believe no plant existing to be more extensively capable of improvement, relatively to the climate of England; and if practical evidence were wanted to prove the extent, to which the culture of the potato is calculated to increase and support the population of a country, Ireland most amply affords it; where population has increased among the catholic poor, with almost unprecedented rapidity, within the last twenty years, under the pressure of more distress and misery, than has perhaps been felt in any other spot in Europe.

Remarks on the curl.

I shall conclude my present communication with some remarks upon the origin and cure of a disease, the *curl*, which a few years ago destroyed many of our best varieties of the potato; and to the attacks of which every good variety of the potato will probably be subject.

Origin of the disease.

I observed that the leaves of several kinds of potatoes, which were dry and farinaceous, that I cultivated, produced curled leaves; while those of other kinds, which were soft and aqueous, were perfectly well formed; whence I was led to suspect, that the disease originated in the preternaturally inspissated state of the sap in the dry and farinaceous varieties. I conceived, that the sap, if not sufficiently fluid, might stagnate in, and close, the fine vessels of the leaf during its growth and extension, and thus occasion the irregular contractions, which constitute this disease: and this conclusion, which I drew many years ago, is perfectly consistent with the opinions I have subsequently entertained, respecting the formation of leaves. I therefore suffered a quantity of potatoes, the produce almost wholly of diseased plants, to remain in the heap, where they had been preserved during winter, till each tuber had emitted shoots of three or four inches long. These were then carefully detached, with their fibrous roots, from the tubers, and were committed to the soil; where having little to subsist upon, except water, I concluded the cause of the disease, if it were the too great thickness of the sap, would be effectually removed; and I had the satisfaction to observe, that not a single curled leaf was produced; though more than nine tenths of the plants, which the same identical tubers subsequently produced, were much diseased.

Experiment to prove the truth of the theory.

In the spring of 1808, Sir John Sinclair informed me, that a gardener in Scotland, Mr. Crozer, had discovered a method of preventing the curl by taking up the tubers before they are nearly full grown, and consequently before they become farinaceous. Mr. Crozer, therefore, and myself appear to have arrived at the same point by very different routes; for by taking his potatoes, while immature, from the parent stems, he probably retained the sap nearly in the state to which my mode of culture reduced it. I therefore conclude, that the opinions I first formed are well founded; and that the disease may be always removed by the means I employed, and its return prevented by those adopted by Mr. Crozer.

Prevention of the disease.

I sent to the Board of Agriculture the substance of the preceding remarks on the origin of the curl, in the year 1808; but I do not know whether that account has been published, or not.

Downton,
January 31, 1810.

XI.

A remarkable analytical Anomaly respectfully submitted to the Consideration of Mathematicians.

To Mr. NICHOLSON.

SIR,

On the algorithm of imaginary quantities.

IN your number for August last, I published a short paper on the defective algorithm of imaginary quantities, which has not at present been honoured by the remarks of any of your correspondents, though the importance of the subject seems to demand the attention of every advocate for the introduction of these expressions into mathematical investigations: and as I am extremely desirous to have the opinion of analysts on this subject, and particularly those, who in their writings have maintained the legitimacy of results, in cases where imaginary quantities have been nearly the only instruments employed in obtaining them; I am induced, in order to draw a reply from those quarters, to consider the same under rather a different point of view. For which purpose let us assume the two following expressions:

$$\sqrt[3]{-2+11\sqrt{-1}}+\sqrt[3]{-2-11\sqrt{-1}}=-4$$

$$\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}+\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}=-1{\cdot}87938$$

which equalities may be verified either by the developement of the above expressions into series, or by the solution of the equations of which they are the roots, according to Cardan's rule, viz.

$$x^3-15x=-4$$
$$x^3-3x=-1$$

Now let us square these formulæ at full length, and by precisely the same steps. Then we shall have the following operations:

$$(\text{A})\ldots\left(\sqrt[3]{-2+11\sqrt{-1}}+\sqrt[3]{-2-11\sqrt{-1}}\right)^2=$$

$$\sqrt[3]{(-2+11\sqrt{-1})^2}+2\sqrt[3]{(-2+11\sqrt{-1})\times(-2-11\sqrt{-1})}$$

$$+\sqrt[3]{(-2-11\sqrt{-1})^2}.$$

Again

$$(\text{B})\ldots\left(\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}+\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}\right)^2=$$

$$\sqrt[3]{(-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3})^2}+2\sqrt[3]{(-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3})\,(-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3})}+$$

$$\sqrt[3]{(-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3})^2}$$

Thus far we have proceeded step by step the same in both examples, and let us still continue the same parallelism of operation at full length thus:

$$\left.\begin{array}{l}-2+11\sqrt{-1}\\ -2+11\sqrt{-1}\end{array}\right\}\text{ the square of }(-2+11\sqrt{-1})$$

$$4-44\sqrt{-1}-121=-117-44\sqrt{-1}=(-2+11\sqrt{-1})^2$$

$$\left.\begin{array}{l}-2+11\sqrt{-1}\\ -2-11\sqrt{-1}\end{array}\right\}\text{ the prod. of }(-2+11\sqrt{-1})\,(-2-11\sqrt{-1})$$

$$4+121\sqrt{-1}=125$$

$$\left.\begin{array}{l}-2-11\sqrt{-1}\\ -2-11\sqrt{-1}\end{array}\right\}\text{ the square of }(-2-11\sqrt{-1})$$

$$4+44\sqrt{-1}-121=-117+44\sqrt{-1}=(-2-11\sqrt{-1})^2$$

and consequently our square (A) becomes

$$\sqrt{-117-44\sqrt{-1}}+2\sqrt[3]{125}+\sqrt[3]{-117+44\sqrt{-1}}=$$

$$\sqrt[3]{-117-44\sqrt{-1}}+10+\sqrt[3]{-117+44\sqrt{-1}}$$

Again, to square our second formula (B)

$$\left.\begin{array}{l} -\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3} \\ -\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3} \end{array}\right\} \text{the square of } \left(-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}\right)$$

$$\tfrac{1}{4}-\tfrac{1}{2}\sqrt{-3}-\tfrac{3}{4}=-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}=\left(-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}\right)^2$$

$$\left.\begin{array}{l} -\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3} \\ -\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3} \end{array}\right\} \text{the product of } \left(-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}\right)\left(-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}\right)$$

$$\tfrac{1}{4}+\tfrac{3}{4}=1$$

$$\left.\begin{array}{l} -\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3} \\ -\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3} \end{array}\right\} \text{the square of } \left(-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}\right)$$

$$\tfrac{1}{4}+\tfrac{1}{2}\sqrt{-3}-\tfrac{3}{4}=-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}=\left(\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}\right)^2$$

And consequently our square (B) becomes

$$\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}+2\sqrt[3]{1}+\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}=$$

$$\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}+2+\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}$$

Thus far likewise we have proceeded step by step in both operations.

And since our first formula is equal to -4, and our second to $-1{\cdot}87938$; the square of the former ought to be equal to $4^2=16$, and the latter to $(1{\cdot}87938)^2=3{\cdot}532069$; that is we ought to find the following equalities obtain: viz.

$$(A)\ldots\sqrt[3]{-117-44\sqrt{-1}}+10+\sqrt[3]{-117+44\sqrt{-1}}=16$$

$$(B)\ldots\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}+2+\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}=3{\cdot}532069$$

Or by transposing 10 and 2

$$(A)\ldots\sqrt[3]{-117-44\sqrt{-1}}+\sqrt[3]{-117+44\sqrt{-1}}=6$$

$$(B)\ldots\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}+\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}=1{\cdot}532069$$

Now

On the algorithm of imaginary quantities.

Now $\sqrt{-117-44\sqrt{-1}} = 3-4\sqrt{-1}$

and $\sqrt[3]{-117+44\sqrt{-1}} = 3+4\sqrt{-1}$

as will be found by involution.

And consequently their sum is equal to 6 as it ought to be, and we may therefore fairly conclude, that we are right in our operation on the first formula. But with regard to our second expression, it is the same as that with which we begun, and is therefore equal to 1·87938, and not 1·532069, as it ought to have been, had we been correct in the operations on the second formula; and hence we may conclude, with equal certainty, that some mistake has crept in unobserved in the latter case, notwithstanding we have proceeded by parallel steps in both examples.

The questions, therefore, that I have to propose to mathematicians, are as follows:

1. What constitutes the errours in the operation on the latter formula?

2. How are such errours to be guarded againſt in other cases?

The latter of these questions is equally important with the former; as there are various other formulæ of a similar description, which, should they arise in any investigation, when we have not the means of checking the result as in the examples above, much uncertainty must necessarily attend the conclusions thence deduced.

The manner in which I have introduced these questions may appear somewhat novel in the present day, but it was not uncommon at the time when the sciences were most successfully cultivated in this country, and when they were making those rapid advances, which have immortalized the names of several distinguished English mathematicians and philosophers.

I have only now to observe, that, should no answer appear to these questions within three months, I will then, through the medium of your Journal, publish my explanation; but I am not without hopes of seeing the subject elucidated by a more able hand than, Sir,

Your obedient Servant,

MATHEMATICUS.

XII.

On the Migration of Swallows: by Dr. TRAILL. *Read before a Literary and Philosophical Society established at Derby, Sept. the 17th, 1808, of which Dr.* TRAILL *is a Corresponding Member.*

To Mr. NICHOLSON.

SIR,

Migration of swallows.

YOUR correspondent Mr. Forster having solicited information on the subject of the migration of swallows, Dr. Traill was induced to request, that the following paper, after having been read to the Derby Society, might be transmitted to you for publication. In compliance with that wish it is herewith enclosed; and, I have no doubt, will be considered as an interesting contribution to this curious branch of natural History.

I am, Sir,
Your very obedient Servant,
Derby. CHARLES SYLVESTER.

Extract from the Logbook of the Ship Jane of Lancaster.— Captain JOHN THOMSON.

On the 17th of May, 1807, in latitude 51° 42′ north; longitude 21° 44′ west. Pleasant clear weather. Wind W. N. W.

18th. Pleasant clear weather. Light airs and calms. Wind varying from S. E. to E. N. E. Lat. D. R. 52° 6′ N.; long. 21° 44′ W.

19th. Steady breeze from E. S. E. Some showers of rain, and foggy weather for the most part of this day. Lat. D. R. 52° 11 N.; long. 21° 10′ W.

Martins and swallows lighting on two ships in the Atlantic in May.

20th. Strong breezes, varying from S. to S. E. Foggy weather. About 4 p. m. several *martins and swallows appeared about the two ships.* At 8 p. m. collected to a large covey; many of which pitched on different parts of this ship, and allowed themselves to be taken up by the seamen. At daylight in the morning found many of them dead in the mizen top, channel bends, and on deck. Lat. D. R. 52° 33′ N. Long. 20° 21′ W.

21st. Continues foggy, attended with rain. Wind mostly from south-eastward. In the course of the day great numbers of the swallows and martins were taken by the seamen; and the cats and dog brought many of them. A great many had perched in different parts of the ship; and all or the greatest part found dead in the morning.

Remarks by Dr. TRAILL.

The intelligent seaman, who made this extract from his logbook at my request, was then on his voyage from the West Indies. He has been many years captain of a ship in the West India trade from Lancaster, and from this port. I know him to be a man of probity and veracity; and his account was confirmed by some of the mariners of the ship then in company, with whom I conversed.

The circumstances chiefly to be attended to in the narration, are:

They were apparently on their passage from Africa to the north, not blown from the land by a storm.

1. The weather, previously, was not so boisterous as to countenance the idea, that the swallows were forced by a tempest from the nearest shore; and the general direction of the wind was not unfavourable to the supposition of their having been aided by it, in their passage from the coast of Africa, where they were observed by the celebrated, but unfortunate, Adanson, to arrive in the winter.

2. The season of the year is favourable to the idea of their migration from the coast of Africa for the north of Europe. They alighted on the ships about the time that swallows begin to appear in Britain, to which they were probably proceeding: and it should not be forgotten, that about this time of the year swallows are seen to quit the coast of Senegal, and other parts of Africa.

3. The debility of these birds, which permitted them to fall an easy prey to the cats and dog; their suffering themselves to be caught by the seamen; and their being very lean, as I was informed was the case by those who examined them in the two ships, seem to show, that they had made a long voyage, and not, that they had been accidentally driven by a gale, from the neighbouring shores of

Britain and Ireland. Indeed, considering the great strength of wing, and velocity, of the swallow tribe, it must have been a tremendous gale that could drive them off the land: but, the previous weather was nothing boisterous, and captain Thomson experienced little more than a steady breeze.

4. The great number of these birds is another argument against the supposition of their having been carried to sea by a storm. Such instances in solitary birds of weak wing are not uncommon. I once caught a golden crested wren (*motacilla regulus*, Lin.) in the shrouds of a vessel, when driven off the coast of Scotland by a sudden tempest; but instances of large flocks of birds, so strong and active as the swallow tribe, becoming the sport of the winds, are certainly very uncommon, even when the weather has been tempestuous.

There were at least two species.

5. Captain Thomson expressly mentions both swallows and martins; and he stated to me, that they differed in size. Hence, there were, at least, two species of swallows observed by him. As he does not pretend to the character of a naturalist, perhaps, there were not only the chimney swallow, or *hirundo rustica*, and martin, or *h. urbica*, but the swift, or *h. apus*, and even the sand swallow, or *h. riparia*. This account, at least, supplies, in some degree, an omission of Mr. Adanson; who, in his interesting observations on the appearance of swallows in Africa, has omitted to state what species he observed there, or whether he observed more than one kind of swallow.

Their being found under water very questionable.

The preceding extract affords, in my opinion, another argument to prove the annual migration of swallows. That swallows sometimes have been found dormant, in the winter season, in cold climates, I am not disposed to deny. But had a bird so common with us generally remained here all the winter in a dormant state, we, probably, should have discovered it more frequently than has ever been pretended. I will even admit, that swallows have been found concealed amid rushes, by the banks of rivers, in this state: but that they have ever been discovered alive at the bottom of pools and rivers, or otherwise excluded from the access of atmospheric air, we must be permitted to doubt, till it is proved, that the respiratory organs of swallows differ from those of other birds; or, that atmospheric air unnecessary to the life of dormant animals. The extraordinary suspension of most of the living functions of animals of this class is a subject of great physiological importance and curiosity; and deserves to be more fully investigated. But the claim of the swallow to an unusual structure of the organs of respiration is completely overturned by the dissections of the celebrated John Hunter. In the alleged cases of the submersion of swallows we must make allowance for the credulity, or inaccuracy, of observers; and I think it would not be difficult to refer almost all such alleged facts to one or other of these heads.

No unusual structure of the organs of respiration in swallows.

Liverpool. THOMAS STEWART TRAILL.

XIII.

Account of the Appearance of a Luminous Meteor: by Professor PICTET*.

Luminous meteor seen at Geneva.

THE 15th of this month, about half after eight in the evening, a luminous meteor was seen at Geneva, in the N. N. W. part of the sky, which was pretty clear where the meteor appeared, though there were clouds in other parts, and the phenomenon itself, toward the end of its appearance, was obscured by a cloud. The appearance was so sudden, that those of the spectators, who were looking another way, at the first moment supposed the light it gave, which was sufficiently vivid to cause a shadow, though it was still twilight, to be the effect of a flash of lightning. We have endeavoured to collect all the particulars respecting the circumstances of the phenomenon, that we could obtain from eye-witnesses of it. Among these may be distinguished five students of the academy, of the faculty of sciences, who happened to be walking together, and not only saw, but made their observations on this phenomenon, which they afterward committed to writing. These, except the noise, which was heard only by them, agree with all those, that have been communicated to us by others with less precision. The following are their words.

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* Bibliothéque Britannique, for May, 1811, p. 105.

"The 15th of this month, at 35 minutes after eight in the evening, we heard a whizzing sound in the north-west. A sudden flash of light caused us to turn our heads, and we saw a kind of serpent of fire, which appeared to us four or five degrees in length. It was bent back at the west end, so as to approach the figure of the letter S; it then spread out in the lower part; after which it assumed the shape of a horseshoe, and nearly of a parabola. At the end of seven or eight minutes, according to our watches, a cloud concealed it from our eyes, at the moment when it appeared to advance very slowly toward the west. Its brightness diminished every instant: and just at the time of its disappearing we no longer perceived any thing but two very bright points, one at the extremity of the lower branch of the parabola, the other on the same branch nearer the summit of the curve. As to its height we can say nothing precise, as we had no instrument with us adapted for measuring angles: but to the eye it appeared twice the height of mount Jura."

Description of it.

One of the eye-witnesses of this phenomenon*, who observed it with a small telescope, remarked, that the most luminous part was not homogeneal, or continuous, but composed of distinct and separate particles.

Composed of luminous points.

A fortunate circumstance enabled us to determine with tolerable precision the important circumstance of the apparent height of the meteor, which was for a long time nearly stationary. Two of its observers†, whom we consulted, remarked that the meteor, seen from a spot which they easily found again, grazed the summit of a certain tree, which even concealed part of its light: We afterward measured from the spot of observation the angle of altitude of this tree, and its azimuth. This altitude, and consequently that of the meteor, was eighteen degrees; and its azimuth was precisely in the direction of the magnetic meridian, which at present at Geneva is 20° 15′ N. W. This direction passes nearly through the zenith of the towns of Gray, Langres, Chaumont, Vitry, Chalons sur Marne, Rheims, Valenciennes, and Bruges.

Its apparent height.

Its azimuth.

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* Mr. Trembley nephew of the celebrated naturalist.

† Mr. L'huilier, professor of mathematics in the academy; and Mr. Galland, student in the faculty of divinity.

We

It was seen at Paris.

Estimation of its real height.

We have learned by the public papers, that this meteor was seen at Paris; but it is not said in what direction. Supposing it to have been seen due east, the intersection of this azimuth with that observed at Geneva would point out the place of the meteor in the region of Vitry, Chalons, and Bar sur Ornain, about seventy leagues in a straight line from Geneva: a distance which, with its apparent observed height, and taking into account the effect of the sphericity of the Earth, would place the meteor about the actual height of twenty-four leagues and half.

The supposition we have made, for want of observations, may serve as a guide to those, who remarked nearly the azimuthal direction of the phenomenon, and would form an idea of its absolute height. It must have been less than 24½ leagues, if the meteor were seen in a direction to the southward of east; and on the contrary so much more, in proportion as it appeared more to the north of the perpendicular to the meridian of Paris.

Above our atmosphere.

At any rate it appears, that its height exceeded the sensible limits of our atmosphere; and that its light, and probably its heat, did not arise, as in our ordinary combustions, from the presence and decomposition of the oxigen gas of the atmosphere.

Probably stones fell from it.

Several circumstances of this phenomenon were similar to those that have been observed in lapidiferous meteors; and we should not be surprised to hear, that incandescent stones had fallen in places, which had this meteor in their zenith. No explosion was heard; but perhaps the distance was too great, and the circumambient medium too rare, for the sonorous vibrations to be transmitted to us.

XIV.

Letter from Professor P. PREVOST, *to Professor* PICTET *on the Meteor of the* 15*th of May**.

GENEVA, *May the* 28*th*, 1811.

Comparison of circumstances.

THE care you have taken, my dear colleague, to determine exactly the position of the meteor observed the 15th of this month, will allow it to be compared with those ascertained by other observers. This is the only method of obtaining any accurate ideas respecting the vertical height, course, and nature, of these bodies foreign to our Earth, and the short passage of which cannot be foreseen.

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* Bibliotheque Britannique, for May, 1811, p. 110.

of meteors desirable.

You have availed yourself of a favourable circumstance, to obtain the altitude and azimuth of that luminous object; but it is far from probable, that observers in other situations should be able to avail themselves of a similar proceeding: and should they report the height of the meteor, without having determined it by any instrument, we must expect great deviations.

Guesses at their heights and distances not to be depended on

There is a certain degree of confidence however, to be given to the estimations of men accustomed to appreciate their sensations, and compare quantities. If therefore such an observer should say, that he saw the meteor nearly at 45°, or at 30°, for example, this might be considered as probably coming pretty near the truth: because we may presume the observer, measuring in idea the interval from the zenith to the horizon, could pretty well estimate by the eye the half or third of that distance.

unless by experienced persons.

But there is a correction to be made in this estimate, which is scarcely thought of, but which, in loose observations of this sort, is in reality of great importance.

Necessary correction in the estimates of these,

The apparent firmament is a skene arch, which may be compared to an arc of a circle of about 60°: (see Smith's Optics, translated by Pezenas, vol. I, p. 117). If we construct a semicircle on a right line, and cut off an arch of about 60° to represent the apparent firmament, (as in the figure in Smith's Optics), we shall see, that half from the vertical of this apparent firmament answers to about 30° of real altitude, and a third to about 40°. Now it is easy to perceive the importance of such a correction, if we would obtain any accurate result from comparative observations, and in particular if we attempted to ascertain a parallax.

from the apparent firmament.

In confirmation of this remark I lay before you the observation of a man possessed of all the faculties calculated to mature his judgment in the estimation of measures. You will there see, that he estimates the height of the meteor between

Estimation by an observer at Geneva.

between 30° and 40°, that is between $\frac{1}{4}$ and $\frac{1}{3}$ of the apparent distance from the zenith to the horizon. These points of division of the apparent firmament answer to 14° and 20°* of real altitude: and that of 18°, which you have measured, is between these two. I ought however to add, that the observer in question ultimately fixed on the estimate of a third, or 30° apparently. But all these determinations are necessarily approximations only.

XV.

Improvement in the Aquatinta Process, by which Pen, Pencil, and Chalk Drawings can be imitated: by Mr. J. HASSELL, *No.* 11, *Clement's Inn*†.

SIR,

Imitations of black lead drawings imperfect.

PERCEIVING the various methods of imitating drawings and sketches in the graphic art fall short of an accurate imitation of the black-lead pencil, I determined on an attempt, some years since, which, after repeated experiments, I flatter myself I have fully established.

The subject may be sketched with a pencil immediately on the copper.

The manner is totally new, and solely my own invention: —by the method I adopt any artist can sketch with a black-lead pencil his subject immediately on the copper, and so simple and easy is its style, that an artist can do it with five minutes study.

No retracing necessary.

By this manner, the trouble in tracing an oil paper, and other retracing on the etching ground is avoided, and the doubtful handling of an etching-needle is done away‡, as the pencilling on the copper is visible in the smallest touch:—It has also another perfection, that, by using a broader instrument it will represent black-chalk, a specimen of which I procured Mr. Munn, the landscape painter, to make a trial of. I have herewith sent the said specimen marked C, and Mr. Munn's name is affixed to the same. This subject he actually drew upon copper, under my inspection, in less than twenty minutes, the time he would have taken, perhaps, to do the same on paper; in fact, it can be as rapidly executed on copper as on paper.

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* A third of the arch of 60° from the horizon would give 20°, and $\frac{4}{9}$ 26° 40′. C.

† Trans. of the Soc. of Arts, vol. XXVIII, p 97. The silver medal and thirty guineas were voted to Mr. Hassell for this communication.

Tracing rag.

‡ Tracing rag should be made of a piece of Irish linen, not too much worn, the surface of which is to be rubbed with another rag dipped in sweet oil, just sufficient to retain a small portion of vermillion or pounded red chalk. This must be placed with the coloured part towards the ground of the plate, and the drawing or tracing laid upon it, which must be traced very lightly with a blunt point or needle.

Black chalk imitated.

It is particularly pleasant for colouring up, to imitate drawings, as the lines are soft, and blend in with the colour, It is a circumstance always objectionable in the common method of etching, that those so tinted can never be sufficiently drowned, nor destroyed, and always present a wiry hard effect.

Particularly adapted for colouring:

It is equally adapted to historical sketching, and might be the means of inducing many of our eminent painters to hand down to posterity their sketches, which, at present, they decline, from the irksome trouble attending the repetition of retracing their performances, and the doubtful handling of the etching-needle, which can never give a sufficient breadth and scope to their abilities.

and to preserve the sketches of painters.

I have, sir, forwarded, in an annexed paper, the different specimens, for the inspection of the gentlemen forming the Society of Arts, &c.

In making my specimens I have thought it necessary to show, that, if by any accident a part might fail, it could be retouched a second time, and oftener if wanted; in this particular its simplicity stamps its use.

Any part capable of being retouched.

To elucidate the foregoing proposition, I purposely caused a part of the distance to fail in specimen A A; this is repaired you will perceive in specimen B, and the sharp touches wanted to perfect the sketch are added.

I beg also to state, it is not the style usually termed *soft ground etching*: that process is always uncertain, cannot be repaired, and will only print about two hundred impressions; whereas the specimens herewith sent will print upwards of five hundred, with care.

Not soft ground etching.

Should the Society for the Encouragement of Arts &c.

deem the subject worthy of their reward, I shall feel proud in communicating its process, and flatter myself the arts and artists will feel a peculiar addition and pleasure in its utility.

Permit me, Sir,
to subscribe myself, with all respect,
Your obedient humble Servant,

JOHN HASSELL,
Landscape Draughtsman, 11, Clement's Inn, Strand.

Process of drawing upon Copper, to imitate Black-lead Pencil or Chalk.

Method of drawing on copper to imitate black-lead pencil, or chalk.

A remarkable good polish must be put on the copper with an oil-rubber and crocus martis well ground in oil; after which it must be cleaned off with whiting, and then rubbed with another clean rag.

You are then to pour over your plate the solution to cause ground, which is made as follows;—

No. 1.—Three ounces of Burgundy pitch.
One ditto of frankincense.

Preparation of the ground.

These are to be dissolved in a quart of the best rectified spirit of wine, of the strength to fire gunpowder when the spirit is lighted.

During the course of twenty-four hours this composition must be repeatedly shaken, until the whole appears dissolved; then filter it through blotting-paper, and it will be fit to use*.

Application of it.

In pouring on this ground, an inclination must be given to the plate that the superfluous part of the composition may run off at the opposite side, then place a piece of blotting paper along this extremity, that it may suck up the

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Grounds.

* The ground in hot weather must have an additional one third of spirit of wine added to it for coarse grounds, to represent chalk; and one half added to it for fine grounds, to represent black lead pencil; and always to be kept in a cold place in summer, and a moderately warm situation in winter.

N B.—If any parts are not bitten strong enough, the same process is to be repeated.

ground that will drain from the plate, and in the course of a quarter of an hour the spirit will evaporate, and leave a perfect ground, that will cover the surface of the copper, hard and dry enough to proceed with.

With an exceeding soft black-lead pencil sketch your design on this ground, and when finished take a pen and draw with the following composition, resembling ink: if you wish your outline to be thin and delicate, cause the pen you draw with to be made with a sharp point; if you intend to represent chalk-drawing, a very soft nib and broad-made pen will be necessary, or a small reed.

No. 2.—Composition, resembling ink, to draw the design on the copper.

Ink for drawing on it.

Take about one ounce of treacle or sugar candy, add to this three burnt corks reduced by the fire to almost an impalpable powder, then add a small quantity of lamp-black to colour it; to these put some weak gum-water*, (made of gum-arabic), and grind the whole together on a stone with a muller: keep reducing this ink with gum-water until it flows with ease from the pen or reed.

To make the ink discharge freely from the pen, it must be scraped rather thin toward the end of the nib, on the back part of the quill, and if the liquid is thick reduce it with hot water.

Varni[illegible]

Having made the drawing on the copper with this composition, you will dry it at the fire until it becomes hard: then varnish the plate all over with turpentine varnish†.

It will now be necessary to let the varnish that is passed over the plate, dry, which will take three or four hours at least;

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* Gum water must be made in the proportion of half an ounce of gum arabic to a quarter of a pint of water.

† Turpentine varnish is composed of an ounce of black resin to an eighth part of a pint of spirit of turpentine; if the weather is excessively warm, it ought to be made with a sixth part of a pint of spirit of turpentine.

[I apprehend there is a mistake here, and that the proportions of spirit should be reversed; as more of the liquid would, no doubt, be required in cold weather than in hot. C.]

but this will depend on the state of the weather: for if it should be intensely hot, it ought to be left all night to harden.

Mode of rubbing off the touches.

Now the varnish is presumed to be sufficiently hard, you may rub off the touches made with the foregoing described ink with spittle, and use your finger to rub them up; should it not come off very freely, put your walling-wax round the margin of your plate, and then pour on the touches some warm water. but care must be taken it is not too hot.

The touches now being clean taken off, wash the plate well and clean from all impurities and sediment of the ink, with cold *soft* water; then dry the plate at a distance from the fire, or else in the sun; and when dry, pour on your aqua fortis, which should be in cold weather as follows:—

Acid.

To one pint of nitrous acid, or strong aqua fortis, add two parts, or twice its quantity of soft water.

In hot weather to one part of nitrous acid add three parts of water.

In every part of this process avoid hard or pump water.

Biting in.

The last process of biting in with aqua fortis must be closely attended to, brushing off all the bubbles that arise from the action of the aqua fortis on the copper.

In summer time it will take about twenty minutes to get a sufficient colour: in winter perhaps half an hour, or more. All this must depend on the state of the atmosphere and temperature of your room. If any parts require to be stopped out, do the same with turpentine varnish and lamp-black, and with a camel-hair brush pass over those parts you consider of sufficient depth; distances and objects receding from the sight of course ought not to be so deep as your fore-grounds: accordingly you will obliterate them with the foregoing varnish, and then let it dry, when you will apply the aqua fortis a second time, and repeat this just as often as you wish to procure different degrees of colour.

Stopping out

Every time you take off the aqua fortis the plate must be washed twice with soft water, and then set to dry as before.

To ascertain the depth of the work.

To ascertain the depth for your work, you should rub a small part with a piece of rag dipped in turpentine, and then apply the finger, or a piece of rag rubbed on the oil-rubber, to the place so cleared, and it will give you some idea of the depth.

Removal of the wax and varnish.

The walling-wax is taken off by applying a piece of lighted paper to the back of the plate, all round the opposite part of the margin where the wax is placed; then let the plates cool, and the whole of the grounds &c. will easily come off by washing the plate with oil of turpentine, which must be used by passing a rag backwards and forwards, until the whole dissolves, it is then to be cleaned off by rags; and care must be taken, that no part of the turpentine is left hanging about the plate.

Printing.

The plate should only pass once through the press.

Sir,

The author's claim to the invention.

During the conference of the Committee of Polite Arts last Monday evening, an Essay on the Art of Aquatinting was produced, which, until that period, I had never seen; since then, I have procured a copy, and carefully perused it. As far as theory goes, respecting aquatinta, I allow it to be fair; but upon the practical part it is positively wrong, and what relates according to the opinion of your Committee as referring to my invention of the imitation of chalk and pencil-drawing, I can prove, by incontestible evidence, that I did produce specimens of my invention as far back as the year 1795 to the public, since which time I have improved the principle.

I flatter myself your goodness will enforce on the minds of those gentlemen who were present, that I ought personally to prove the same, which I am prepared with documents to do.

Permit me, Sir, to remark, after a lapse of fifteen years, that surely some person might have produced figures and landscapes sketched in this manner; but not a single artist, to my knowledge, ever gave one specimen to the public except myself, though my examples have been before them all the above time.

It is upon the application of the manner for freedom of imitating drawings, that I conceive it to be of importance, and

The author's claim to the invention.

and from this circumstance in pointing out its utility, I claim a credit from its originality. If, Sir, it was previously known, why was it not in use? The fact appears to me, that no person, except myself, thought of taking the pains to study the subject.

Having thus brought it publicly to notice, I still feel a degree of pride in furnishing an additional and easy step to the promotion of the arts.

I have now, Sir, to apoligize to you for trespassing on your patience, and as it is not possible for any gentleman to have taken more trouble, or have paid a more polite attention to the circumstance, I thought it most decorous to submit this memorial to you, as one of the Chairmen of the Committee of Polite Arts.

Trusting, Sir, you will be so good as to communicate the same to the Committee, I beg to subscribe myself, with all respect,

Sir,
Your very obedient humble Servant,
J. HASSELL.

No. 11, *Clement's Inn, May* 10, 1810.

To J. T. Barber, Esq.
A Chairman of the Committee of Polite Arts.

XVI.

On the Nature of Oximuriatic Acid Gas, and the Conversion of Carbonic Oxide into Carbonic Acid by it, in Reply to Mr. J. Davy. *In a Letter from Mr.* J. Murray, *Lecturer on Chemistry, Edinburgh.*

To MR. NICHOLSON.

SIR,

I Have not seen until lately Mr. J. Davy's communication in your Journal, for September last, and I embrace as early an opportunity as occurs to me, of offering a few observations in reply to it.

New gas sup-

The most important part of this communication is that, which posed to account for the production of carbonic acid.

which relates to what he considers as a new gas, the operation of which, he supposes, serves to account for the production of carbonic acid, which I have found to be the result of the mutual action of oximuriatic acid, carbonic oxide, and hidrogen gasses. It is produced, he states, by exposing a mixture of equal volumes of carbonic oxide and oximuriatic gas to light, and he regards it as a compound of these two gasses.

Not produced in an experiment of Mr. Murray's.

I had already performed this experiment without obtaining the results he has described; and I am not aware of any fallacy, by which this can be accounted for; there was no sensible production of carbonic acid (the point I had it more particularly in view to ascertain by the experiment) and after agitation with water to remove the oximuriatic gas, the carbonic oxide was recognized by burning with its usual blue lambent flame, and forming carbonic acid by its combustion.

Oximuriatic and carbonic oxide gasses do not act on each other

This result of the nonaction of oximuriatic gas on carbonic oxide gas, when both are perfectly dry, has been lately asserted still more strongly by Gay-Lussac and Thenard, and the terms they employ are even unusually decided. After observing, that the carburetted hidrogen gasses are acted on by oximuriatic acid gas when exposed to light, they add " mais à quelque dose qu'on ait mêlé le gaz acide muriatique oxigéné sec, et le gaz oxide de carbone preparé avec le fer et le carbonate de barite, quelque fort qu' ait été la lumière à laquelle on les a exposés, enfin quelque long qu' ait été le contact, il n'y a point eu d'action*". If I have been deceived therefore, it is in common with chemists of the highest reputation for the accuracy and delicacy of their experimental researches. These circumstances however lead me rather to believe, that there is some peculiarity necessary to the success of Mr. J. Davy's experiment. I know sufficiently the disadvantage to which any experi-

unless possibly under peculiar

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* Recherches Physico Chemiques, T. 2d, p. 192.

[" But in whatever proportions we mixed dry oxigenized muriatic acid gas, and carbonic oxide gas procured by means of iron and carbonate of barytes, however strong the light to which they were exposed, and lastly however long they remained in contact, no action between them took place." C] circumstances.

mentalist is subjected, who undertakes the examination of experiments of which only a general account is given; and, from both these considerations, I am induced to suspend any experimental investigation of this subject until the more full account, which Mr. J. Davy announces he is to give of his experiments, is published. At present, I shall admit the production of this new gas, and shall offer merely a few observations on its relation to the present controversy.

Carbonic oxide converted into acid by oximuriatic gas,

In my first communication I had stated, that, when oximuriatic acid, carbonic oxide, and hidrogen gasses are submitted to mutual action, the carbonic oxide is converted almost intirely into carbonic acid. This result, inconsistent with Mr. Davy's hypothesis of the nature of muriatic and oximuriatic acids, was attempted to be explained by the assumption, that a portion of the water introduced to absorb the product of the action of the gasses had suffered decomposition, and that from this oxigen had been communicated to the carbonic oxide, so as to convert it into carbonic acid. Messrs. Davys, therefore, in repeating these experiments, employed ammonia to condense the product, and with this variation they found the carbonic oxide to remain unchanged.

alleged to be from the decomposition of water.

This disproved.

Though satisfied, that there was no probability in this assumption of water being decomposed, I thought it proper to repeat the experiment with the variation of condensing the product by ammonia. The result was still the same as that which I had before obtained. Nearly the whole of the carbonic oxide had disappeared, and a concrete salt was obtained, which effervesced strongly on the contact of a diluted acid, and also gave indications of the presence of carbonic acid by the test of muriate of barytes. I concluded therefore, as I believe any chemist would have done from these results, "that the production of carbonic acid in this experiment "was established beyond the possibility of doubt."

The same result obtained by Mr. J. Davy.

Precisely the same results have now been obtained by Mr. J. Davy. Repeating my experiment on the exposure of the mixture of the three gasses to light, he detected, "after the addition of ammonia, no traces of carbonic oxide": and he perceived, as I had stated, "an effervescence of the ammoniacal salt formed with nitric acid:" an effervescence which he farther admits to be owing to the disengagement of carbonic acid. The dispute therefore with regard to the fact is at an end; and the production of carbonic acid in these experiments, which I had always maintained to be the result, but which Messrs. Davys had denied, is established beyond the possibility of doubt.

His conclusion

Mr. J. Davy, however, forms a singular conclusion with regard to this. Having stated the results of his experiments, he adds: "after the preceding statement of facts, Mr. Murray, I should conceive, will be induced to renounce his conclusion, that the production of carbonic acid in his experiment was established beyond the possibility of doubt; and admit, that what he considered as carbonic acid was actually the new gas just described; and I should likewise imagine, that this gentleman, in future, will be more cautious in his assertions and criticisms on the labours of others." It is but justice to Mr. J. Davy to state on what grounds these expectations are founded.

grounded on the supposed formation of a new acid.

The result of the experiment with carbonic oxide, oximuriatic acid, and hidrogen gasses led him to repeat the experiment with the two former gasses alone. Having exposed therefore a mixture of carbonic oxide and oximuriatic acid without hidrogen to light, he obtained a similar result, a total condensation by ammonia without the slightest remains of carbonic oxide. By farther researches he found, that an acid gas is formed from the mutual action of the oximuriatic and carbonic oxide gasses, which combines with ammonia, and forms a concrete salt, and from the agency of this gas he explains the production of carbonic acid in my experiments. "I have now to announce", he remarks, "the existence of a new acid gas, which operated in Mr. Murray's experiments without his knowledge of its presence, and was the cause of those phenomena, which he erroneously attributed to the formation of carbonic acid gas." He supposes it to combine with the ammonia which is added, and to form a concrete salt; and "the decompo-"sition," he adds, "of this ammoniacal salt with effer-"vescence by dilute nitric acid deceived Mr. Murray."

On reading this paragraph I expected it to be proved, that no carbonic acid is disengaged from the concrete salt, and that the effervescence was found by Mr. J. Davy to be owing to the disengagement of this new acid gas. Then indeed, he would have had reason to say, I had been deceived; and grounds to form the expectation, that I should renounce my conclusion, that carbonic acid had been formed in my experiment; but in the succeeding sentence I found, sufficiently to my surprise, the admission, that it actually is carbonic acid, which is disengaged with effervescence, that my conclusion therefore is correct, and established by Mr. J. Davy's own experiments; and all that his labours amount to is, that by the aid of this gas he can frame an *hypothesis*, by which this production of carbonic acid, hitherto so steadily denied by him, may now, that he admits it, be accounted for in conformity to the opinion he defends.

The first question is the fact of the formation of carbonic acid,

It is obvious, that the first question in the controversy is with regard to the matter of fact. Is carbonic acid formed in these experiments, or not? How it is formed is a different question. I had uniformly maintained its production, or, that when carbonic oxide, oximuriatic acid, and hidrogen gasses are submitted to mutual action, the carbonic oxide disappears; and, whether the product be examined by the medium of water, or of ammonia, carbonic acid is obtained. Mr. J. Davy denied this. But it now appears from his own experiments, that my statement has been correct, that the carbonic oxide does disappear, and that carbonic acid is obtained. He therefore, I trust, will in future be more cautious in his assertions, and in calling in question the results of the experiments of others.

which is proved by Mr. J. Davy's own experiment.

His hypothesis to explain this.

The hypothesis, which he proposes to account for the facts now admitted, is the following. The carbonic oxide he supposes to unite with the oximuriatic acid, and form this new acid gas; it combines with the ammonia, and in the decomposition of this ammoniacal salt with effervescence, "water is decomposed, its hidrogen is abstracted by the oxi-"muriatic acid to form muriatic acid, and its oxigen by the "carbonic oxide to produce carbonic acid, which is disen-"gaged."

* One would imagine from the manner in which the above sentence is expressed, that these were facts which had been

experimentally ascertained. They are however a series of suppositions, some of them in opposition even to the evidence, which Mr. J. Davy brings forward.

No proof that the new gas is formed in this experiment.

Thus no proof is given, that this new gas had been formed in the experiment. Admitting it to be formed when oximuriatic acid and carbonic oxide gasses are submitted to mutual action; it does not follow, that it will also be formed when they are in mixture with hidrogen. We know it is not formed when a little water is admitted, but that the products in this case are muriatic and carbonic acids. It is equally possible, that hidrogen may modify their mutual action so as to prevent its formation; that in this case also these acids are formed on the principle I have already explained; that the concrete salt formed with ammonia consists of muriate and carbonate of ammonia, and that the carbonic acid is directly disengaged from this salt by the diluted acid. There is not a single phenomenon attending the experiment as stated by Mr. J. Davy, which does not accord with this explanation.

Farther supposition, that this gas is capable of decomposing water:

It is farther an hypothesis, that this new gas is capable of decomposing water, when disengaged by an acid from its combination with ammonia; an hypothesis assumed to account for the production of carbonic acid, and supported by no proof. Mr. J. Davy says, indeed, that it must "appear evident, when it is known, that this new gas neither inflames on the passage of the electric spark with either oxigen or hidrogen alone, but that it detonates violently with a mixture of oxigen and hidrogen in proper proportions, and affords muriatic and carbonic acid gas." It is sufficiently evident, however, admitting even Mr. J. Davy's idea of the composition of this gas, that, when these gasses are in mixture with it, each of them exerting an affinity to one of its ingredients, without any affinity being exerted between them to counteract this, these combinations may be established; while it does not follow, that, when the oxigen and hidrogen aré united by a strong affinity as they are in water, this will be overcome, and the water be decomposed. But why have recourse to these remote and indirect considerations? Let the fact be at once appealed to; does this gas decompose water or not? It appears from Mr.

the contrary of which appears.

Mr. J. Davy's own account, that it does not; he states merely, that it is very slowly absorbed by water. It is therefore directly in the face of experimental evidence to assume, that, when it is disengaged from its combination with ammonia by an acid, it is capable of decomposing water; his hypothesis to account for the disengagement of carbonic acid falls to the ground, and the obvious conclusion must be admitted, that the carbonic acid has been formed by the mutual action of the carbonic oxide, oximuriatic acid, and hidrogen gasses, and that it exists in the concrete ammoniacal salt.

Mr. J. Davy's complaint of an expression of Mr. Murray's.

Mr. J. Davy will now perhaps perceive, that it was with some justice, that I maintained the fact of the production of carbonic acid in these experiments, and that I did not consider it invalidated by what was stated in opposition to it. He complains of an expression, which I employed in the discussion on this point, that "Messrs. Davys did not obtain carbonic acid in their experiments, because they did not look for it with sufficient care, or were not sufficiently aware of the sources of fallacy, by which its production might be concealed." It would be easy to justify this, not only from the results of my own experiments, in which carbonic acid was uniformly formed, results now proved by Mr. J. Davy's evidence to be correct; but from a review of the manner in which the results of the experiments to which I allude were examined. This I decline, however, as an invidious task, unless urged to it by Mr. J. Davy, referring rather to the brief observations, which I have occasionally offered on some of these experiments. Nor should I probably even have used this expression, had it not appeared to me called for by the tone, which has been assumed in this controversy, and the manner in which it has been conducted. If Mr. J. Davy will look back on its commencement, he will find, I believe, my first paper written with a degree of candour, to which it is not in his power to make a single objection. It was impossible, if an opinion were at all to be called in question, to have done so with more calmness and forbearance. Mr. J. Davy thought proper to take up the controversy in a very different spirit and style, and rendered it necessary for me sometimes to introduce a remark, which I should otherwise have avoided.

Of the other parts of Mr. J. Davy's communication I may avoid, I believe, taking any notice. He has prefixed a kind of view of the progress of this discussion, in which are much repetition of what has been already replied to, and misstatements, which to those who have attended to the question it cannot be necessary to obviate. I shall merely give one example of this, and dismiss a subject sufficiently irksome. Mr. J. Davy has found, that, when a mixture of carbonic oxide, hidrogen, and oximuriatic gasses is inflamed by the electric spark, two measures out of ten of the carbonic oxide disappear; and this, he says, I "consider in my last communication as a demonstration, that oximuriatic gas is a compound of an unknown basis and oxigen". There is not a sentence in that communication of mine, that will fairly admit of such an interpretation, nor should I have thought of resting any demonstration on so narrow a basis. I considered the fact established by my own experiments, that there is a total or nearly a total conversion of carbonic oxide into carbonic acid, as such a demonstration. I have farther considered this partial conversion of carbonic oxide into carbonic acid in Mr. J. Davy's experiment, as *a confirmation to a certain extent of my results*; and I pointed out to him a very sufficient reason, why its success had not been more complete—his having diminished the proportion of hidrogen to less than one half of that which I had employed. It is not more necessary perhaps to take notice of his remarks with regard to the action of oximuriatic gas on carburetted hidrogen. He must have known of the difference of opinion, which prevails among chemists with regard to the carburetted hidrogen gasses, and of course, in giving an account of any experiments upon them, he ought to have mentioned what particular gas he employed. The gas from humid charcoal has been regarded as a variety of carburetted hidrogen, it is the one even to which the name was first given, and to which it is still applied; and though different opinions exist with regard to its constitution, I could not know what opinion Mr. J. Davy held with regard to it, or what he considered as exclusively carburetted hidrogen. The subject however is one of little importance, and my observations with regard to the one gas will

Instance of a misstatement by Mr. J. Davy.

Mr. J. Davy does not say what kind of carburetted hidrogen he used.

will still, I believe, hold just with regard to the other, nor would there be any difficulty in showing the imperfections of Mr. J. Davy's experiment.

Mr. Davy's explanation of phenomena hypothetical and unproved.

The question with regard to the general merits of the subject, I conceive now to be at rest. Mr. Davy's opinion, which was first held out as a genuine theory, admitting of no doubt as being a simple expression of facts, has been shown to be a hypothetical explanation of phenomena. And as an hypothesis not a single proof has been given of its truth, or no fact has been brought forward, exclusively explained by it, or explained with more probability than by the opposite hypothesis. It requires in its adaptation to the phenomena more multiplied and complicated assumptions, and it is at variance with the most strict and extensive analogies.

Mr. Murray's opinion agreeable to that of Berthollet, Gay Lussac, and Thenard.

I am pleased to find my opinion on this point sanctioned by that of Berthollet, and of Gay-Lussac and Thenard. That by the latter chemists is of too great a length to perpermit me to introduce the quotation. I therefore refer to their memoir*. Berthollet, in a report on their researches, has given a more condensed view, equally clear and candid, his opinion cannot be received without interest by chemists, and you may therefore perhaps find room for the insertion of it. After remarking, that Gay-Lussac and Thenard had concluded, from their experiments, that oximuriatic acid gas may be a simple substance, and that all the phenomena it exhibits may be explained on that hypothesis, but that they had preferred the common hypothesis, as explaining them still better, a preference they continue to give notwithstanding the other idea has been adopted by Mr. Davy; Berthollet adds,

Berthollet's remarks on Mr Davy's hypothesis;

"In fact, to consider the oxigenized muriatic gas as a simple substance, we must suppose, that common muriatic acid is a compound of hidrogen and oxigenized muriatic acid; and that the metallic muriates are of a nature entirely different not only from other metallic salts, from these very muriates themselves dissolved in water. We must suppose, that lime and magnesia give out oxigen, the existence of which in them is supported by certain experiments, according to another hypothesis, to combine in the metallic state with oxigenized muriatic gas; and that this gas combines with the oxigen, which the water gives up to it, to pass to the hyperoxigenized state: and these suppositions are not sufficient to explain every thing.

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* Recherches Physico-chimiques, T 2d, p. 165.

on the former hypothesis,

"In the other hypothesis, that is to say, admitting that oxigen is capable of combining with muriatic acid, as it is with metals, and with all combustible substances, all the explanations are natural, and perfectly analogous with those given of other facts, in which oxigen is transferred from one substance to another. Only the new observations show, that, to effect the change of oxigenized muriatic gas into muriatic gas, it is necessary for the latter to be in a situation to receive the quantity of water necessary to its constitution; which agrees with the force of its combination, which is very great in muriatic acid.

and on hypotheses in general.

"It may not be useless to remark, that, when we discuss the nature of substances, the mode of their combination, and the changes that may take place in the elements that enter into their composition, it is easy to multiply hypotheses: but those that are best supported by analogy, and require the fewest suppositions to connect them with the facts, so that the mind readily embraces their relation to them, should be adopted; still however not confounding their applications with the facts themselves confirmed by weight and measure, or with the inductions that immediately flow from these*."

New experiments promised.

A few months ago I commenced a train of experimental investigation, different from that which I have hitherto prosecuted, which promised to be decisive with regard to these hypotheses. The results of the experiments I have performed have accordingly been such as appear to me to establish the truth of the common opinion. An account of these will, with your permission, form a communication for the succeeding number of your Journal.

I am, with much respect,
Edinburgh, 17th Oct. Your most obedient servant,
1811. JOHN MURRAY.

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*The quotation in Mr. Murray's paper was in the original French: but for the sake of those of our readers, to whom that language is not sufficiently familiar, it is here given in English. C.

XVII.

XVII.

METEOROLOGICAL JOURNAL.

	Wind	Pressure.			Temperature.					
		Max.	Min.	Med.	Max	Min	Med.	Evap.	Rain	
9th Mo.										
Sept. 9	E	30·17	30·15	30·160	75	51	63·0	—		☽
10	E	30·15	30·05	30·100	77	46	61·5	—		
11	Var.	30·15	30·00	30·075	80	55	67·5	·45		
12	E	30·19	30·15	30·170	73	53	63·0	—		
13	E	30·11	30·01	30·060	71	40	55·5	—		
14	E	30·02	29·98	30·000	74	45	59·5	·56		
15	N E	30·07	30·02	30·045	70	55	62·5	—		
16	E	30·05	29·98	30·015	66	44	55·0	—		
17	E	30·05	29·95	30·000	70	47	58·5	—		●
18	E	29·95	29·87	29·910	71	44	57·5	·95		
19	S E	29·87	29·50	29·635	74	47	60·5	—		
20	S E	29·53	29·50	29·515	74	51	62·5	—	—	
21	S	29·80	29·53	29·665	66	54	60·0	·32	·08	
22	Var.	29·80	29·60	29·700	65	52	58·5	—	—	
23	S W	29·60	29·43	29·515	61	43	52·0	—	·14	
24	N W	29·62	28·92	29·270	64	49	56·5	·30	·14	
25	W	29·26	28·86	29·060	60	48	54·0	·10	·46	☾
26	W	29·21	29·17	29·190	61	39	50·0	—	·10	
27	S W	29·33	29·20	29·265	51	40	45·5	·18	·45	
28	N W	29·54	29·33	29·435	62	44	53·0	—	—	
29	N W	29·71	29·54	29·625	64	51	57·5	—	·20	
30	S W	29·72	29·55	29·635	62	50	56·0	·22	·16	
10th Mo.										
Oct. 1	W	29·76	29·47	29·615	63	46	54·5	—	·04	
2	S W	29·87	29·85	29·860	64	39	51·5	·19		○
3	E	29·85	29·46	29·655	61	52	56·5	—	·26	
4	S	29·57	29·46	29·515	69	59	64·0	—	·02	
5	S W	29·75	29·57	20·660	67	54	60·5	·35	·15	
6	S W	29·95	29·87	29·910	63	52	57·5	—	·18	
7	S W	29·92	29·85	29·885	69	54	61·5	·21	·01	
8	S W	30·00	29·90	29·950	67	53	60·0	·10		
		30·19	28·86	29·736	80	39	57·85	3·93	2·39	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

Ninth Month, 9. Before sunset, after a serene day, *cirrus* clouds, pointing downward, from the W. 11. *Cirro, cirrocumulus*, some dry haze: wind westerly by night, scarce sensible. 14. *Cirri* and haze in the evening twilight of a bright orange colour. 15. Much wind: clear. 16. a. m. overcast: p. m. clear: twilight duller, with *cirrostratus*. 17. Much wind: very clear sky. 18. As yesterday: evening twilight luminous, orange, surmounted with rose colour, the latter somewhat in converging streaks. 19. Morning twilight obscure, with dense *cirri*: much dew: wind, a. m. N. E. Thunder clouds at different heights, some of which moved from the S. E. There were clouds throughout the night, with lightning. 20. Wind a. m. N. E. Thunder clouds again, which grouped, and passed about 2 p. m. to the W. with a few drops: *nimbi*, with a faint bow in the distance: evening cloudy, with two strata: wind S. E.: much lightning in the S. W. 21. a. m. Cloudy. Rain, with distant thunder at one and two p. m.: *Nimbi* and *cumulostratus*: faint bow. 22. a. m. Overcast. Wind veered to N. W., apparently by E. *Cirri*, in lines from N. E. to S. W. 23. a. m. Wind fresh from S. W., with rain: p. m. fair, with various modifications of cloud, which were finely coloured at sunset in the east. 24. a. m. Clear: much dew: fair day, but with clouds indicating rain: twilight milky, with a blush of red: the moon disappeared early, behind *cirrostratus clouds*, and it rained heavily in the night. 25. Cloudy and windy, with rain. 26. a. m. *Cirrus* with *cumulus*: p. m. showers. 27. Windy: wet. 28. a. m. Misty: p. m. showers, *cirrostratus*, and a blush on the twilight. 29. Evening, lightning: wet night. 30. Lunar halo.

Tenth month, 1. a. m. Wind S. E. showery. 2. A little before sunrise I observed a *stratus* in the marshes to the S. E., very nearly resembling a sheet of water; one which was seen from this village, in similar circumstances, about two weeks since, was actually taken by several persons for an extensive inundation. In the afternoon, large elevated *cirri* and *cirrostrati* rapidly passing at sunset from red to gray, indicated a renewal of the wet weather. 3. Misty morning, with *cirrostratus* above: very wet, p. m. 4. Much wind: cloudy night. 5. Squally. 6. a. m. Cloudy, much wind: evening calm; large *cirri* and *cirrostrati*, with a blush on the twilight: a bright blue meteor in the N. W.: wet night. 7. Cloudy, with a gale of wind. 8. Fair.

RESULTS.

Barometer: highest observation 30·19 inches; lowest 28·86 inches; range 1·33 inches.
Mean of the period 29·736 inches.
Thermometer: highest observation 80°; lowest 39°; range 41°.
Mean of the period 57·85°.
Evaporation 3·93 Inches. Rain 2·39 inches.

From the full moon of last period to the new moon of the present, easterly breezes with clear days, and the *stratus* by night. Evaporation went on increasing as the wind became stronger: dew fell in plenty, and the small meteors, called shooting stars, were abundant. The latter half of the present period brought the accustomed compensation, in rain from the westward: the approach of this was perceptible for several days beforehand; and the ground being dry, it was attended at the beginning with some discharges of electricity from the clouds.

Several persons, imagining they perceived something extraordinary in the weather, have enquired, whether the present *comet* could have any influence upon the seasons. It would be idle to reason upon its power without proof of its effects; and these, again, must be proved to extend, at least over the whole northern hemisphere; for which a corner of our little island is no adequate standard. It seems within the limits of possible conjecture to say, that comets *may* induce some change in the atmosphere of the planets, by changing the state of the æther (if there be any such medium,) interposed between these and the sun; or by affecting the production of luminous matter on the surface of the sun itself. A comet approaching near to a planet would also disturb the atmosphere of the latter by the mere effect of its attraction: but we have a planet attendant on the Earth, which is doing this every day, and we are still unprepared duly to appreciate its power. Comets are, therefore, at present, out of the province of the meteorologist.

L. HOWARD.

PLAISTOW, *Tenth Mo.* 16, 1811

XVIII.

Experiments on the acid Phosphate of Potash: by Mr. Vauquelin[*].

Crystallizable compound of phosphoric acid and potash.

MR. Vitalis, secretary to the academy of sciences, letters, and arts at Rouen, and professor of chemistry in that city, having formed, in the course of his operations, a compound of phosphoric acid and potash, each extremely pure; and having obtained, by suitable evaporation, a perfectly crystallized salt; presumed that other chemists, who have all announced the uncrystallizability of phosphate of potash, were deceived.

Too modest to take on himself to contradict what had been said on this head by the ablest chemists, he sent me a small quantity of the salt, that I might examine it, and give him my opinion of it. The following are the results of my researches.

Its properties.

1. This salt is very white, crystallized in prisms with four equal sides, and terminated b pyramids with four faces corresponding to the sides of the prism.

2. It has a very sour taste, and powerfully reddens infusion of litmus. It is not alterable by the air.

3. With lime-water it throws down a copious, white, flocculent, and as it were gelatinous precipitate.

4. Caustic potash evolves from it no ammonia.

5. It forms a copious precipitate with solution of muriate of platina.

6. It gives out no phosphorus by the action of heat, but it melts into a clear glass, which crystallizes and becomes opake on cooling.

7. After having been thus melted, it does not dissolve in water so easily as before.

Uncrystallizable when neutralized.

8. A portion of this salt having been saturated with potash, and subjected to spontaneous evaporation, did not crystallize: it was reduced to a kind of viscous liquor, resembling a solution of gum.

A superphosphate of potash.

From these experiments it evidently follows, that the salt in question is an acid phosphate of potash; consequently, that what chemists have said of the common phosphate of potash is not affected by the properties it exhibits: and that Mr. Vitalis has enriched chemistry with a new species of salt, to be placed in the class, already very numerous, of these substances.

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[*] Ann. de Chim. vol. LXXIV, April, 1810, p. 96.

SCIENTIFIC NEWS.

Lectures at the Scientific Institution.

A Card has been transmitted to the subscribers to the Scientific Institution, Princes' street, Cavendish square, announcing the commencement of the annual Lectures at that Establishment, on Tuesday the 19th of November.

The arrangement embraces the following subjects:

A popular course of twelve Lectures, on the most interesting branches of Experimental Science, by Mr. George Singer: a course on the Philosophy of the Mechanic Arts, by Mr. E. Lydiatt: and a course of twelve Lectures on Chemistry, by Mr. George Singer.

Surry Institution, Blackfriars Bridge.

Lectures on natural philosophy, chemistry, music, and belles lettres.

The annual courses of Lectures at this Institution will be delivered in the following order, viz.

1. On the Philosophy of Physics, by I. M. Good, Esq. F. R. S., Mem. Am. Phil. S., and F. L. S. of Philadelphia; to commence on Friday, Nov. 22, and be continued on each succeeding Friday.

2. On the Belles Lettres, by Edward Quin, Esq. to commence on Tuesday the 26th Nov., and be continued on each succeeding Tuesday.

3. On the Chemical Phenomena of Nature and Art, by Fred. Accum, Esq., M. R. I. A., F. L. S.; to commence early in 1812.

4. On Music, by W. Crotch, Mus. D., Professor of Music in the University of Oxford; to commence early in 1812.

Mr.

Lectures on manufactures.

Mr. Clennel, Conductor of the "new Agricultural and Commercial Magazine, or General Depository of Arts, Manufactures, and Commerce", commences a course of six weekly Lectures on Manufactures, at Stratford, near Bow, on the 1st of November. Iron, coal, wool, cotton, linen, and silk, with the various arts and manufactures arising out of or connected with them, will form the leading topics of these discourses, which are intended to be amusing, as well as instructive.

Mathematical papers in the Ladies' Diary.

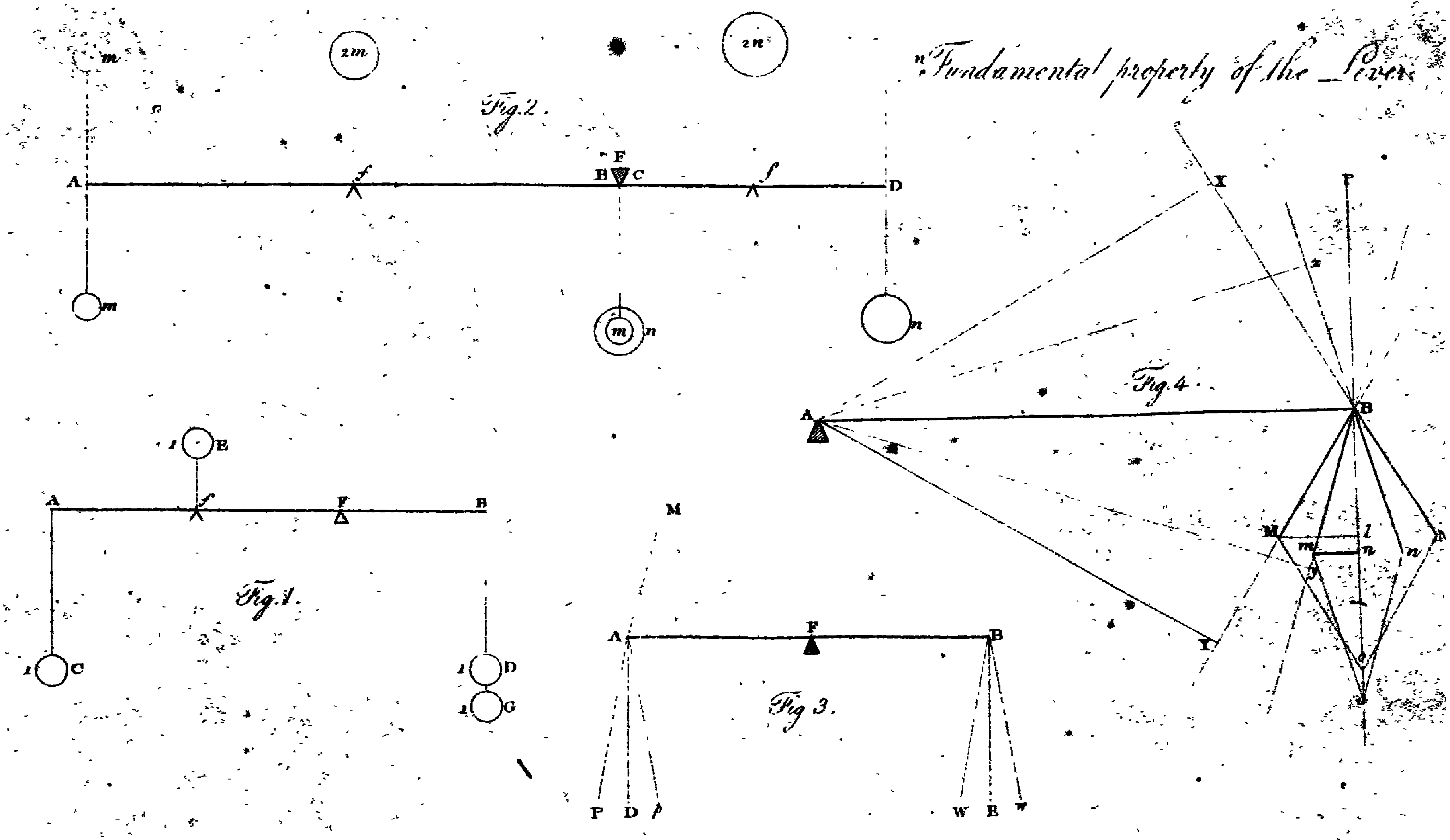
Mr. T. Leybourn, of the Royal Military College, Editor of the Mathematical Repository, intends to publish by subscription a Collection of all the Mathematical Questions, and their Answers, which have appeared in the Almanack called the Ladies' Diary, from its commencement in 1704 to the present time. The editor of the Diary (Dr. Charles Hutton) published a similar work in 1773, but comprehending both its mathematical and poetical parts down to that period. Mr. Leyburn's publication will comprehend only the mathematical part, and, with Dr. Hutton's permission, will contain all the valuable additions given in his edition, as far as it extends. He also hopes to be able to give other additions by the assistance of some of the ingenious mathematicians, who have for a number of years past contributed to the Mathematical Repository.

The work will be printed in 8vo, and will be published in half volumes, one of which will appear every three months. The diagrams will be printed in the text, from figures cut in wood. It will be put to press as soon as such a number of subscribers can be obtained, as shall give the editor a prospect of being indemnified for the expense, which must attend its publication.

Parkinson's Organic Remains.

The 3d vol. of Mr. Parkinson's Organic Remains of a former World is promised in the course of November.

Fundamental property of the Lever



Iron Spire at Edgeworthstown

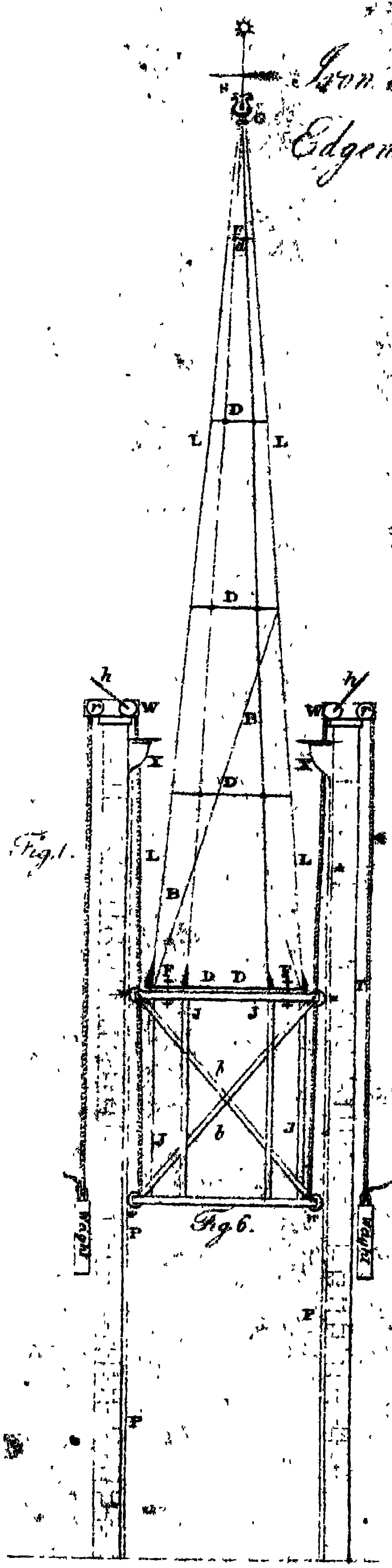


Fig. 1.

Fig. 6.

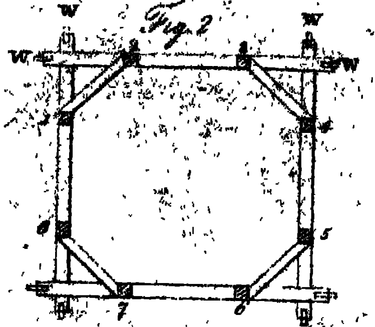


Fig. 2

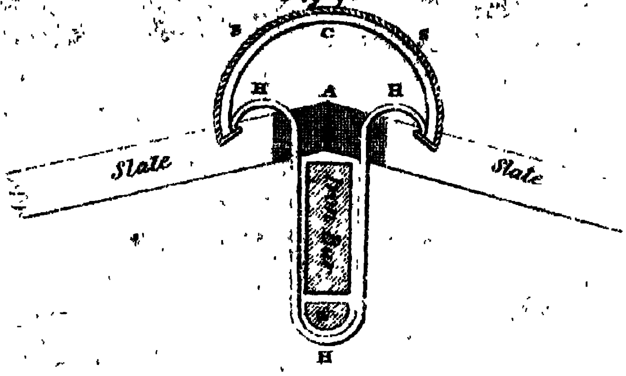


Fig. 7.

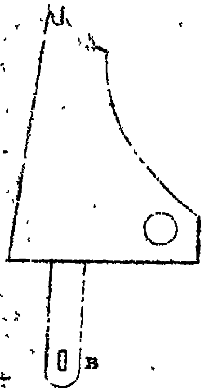


Fig. 4.

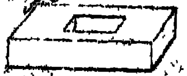


Fig. 9.

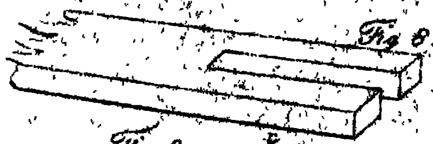


Fig. 8

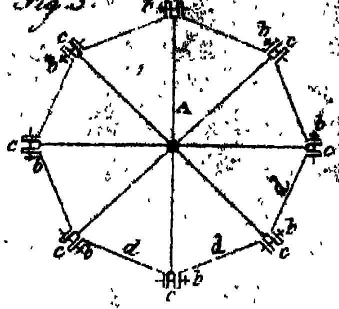


Fig. 3.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1811.

ARTICLE I.

Description of a Spire of a new Construction, at Edgeworthstown, combining the Advantages of Cheapness, Elegance, and Durability. In a Letter from RICHARD LOVELL EDGEWORTH, *Esq., F.R.S. M.R.I.A. &c.*

To W. NICHOLSON, Esq.

SIR,

EDGEWORTHSTOWN, IRELAND,
Sept. the 22nd, 1811.

Spire of a new construction at Edgeworthstown.

I Have lately erected a spire of a new construction on the tower of the church of Edgeworthstown, and, as the attempt has succeeded, I hope an account of it will be acceptable to your readers.

My object was to lessen the expense, and to facilitate the means of ornamenting places of public worship.

Height of the spire.

Mode of its construction.

This spire is fifty feet high from the base to the star by which it is crowned. See Plate VII, fig. 1, which is a section of the tower, the spire, and part of the machinery. The spire was made withinside of the tower, and, when completely finished, was drawn up in a few minutes by machinery, and placed on the tower, where it now stands. It consists of a skeleton of hammered English iron, covered with strong Welsh slates, capped where they meet on the skeleton by large copper beading, which, with the slates, is fastened to the skeleton by copper bands and cramps. The whole is well painted, and covered with sand, so as to imitate stone.

The skeleton.

The skeleton was formed of eight bars of iron, 45 feet long, 2 inches and ½ broad, and ¾ of an inch thick. The dimensions were chosen because they are those of common bars, that are sold by ironmongers. These bars are usually 14 or fifteen feet long, and I had them welded in a common forge to the length that was requisite. Eight of these were disposed octagonally upon a base, fig. 3, about 9 feet in diameter, which is nearly the diameter of the tower. It was made of bar iron an inch square.

Manner in which the parts were fitted together.

Before the spire was put together in the tower, the parts were previously fitted on the ground, not perpendicularly, but lying sideways, so that each bar could be easily reached by the workmen. With this view I took advantage of a saw-pit, which permitted half the base to lie below the ground, while the apex, or point of the spire, was supported by a bench, on the surface of the ground. This enabled me to assemble and fit the bars which were necessary for cross braces, and to combine the bars accurately round the spindle of the weathercock, and to secure them by a ring of iron.

The base

The base above mentioned, fig. 3, consisted of four bars of iron, flattened where they crossed each other, with a hole through the middle of each, that received a bolt to bind them together. The ends of each of these bars were so formed, with cheeks, as to permit the bars, that composed the spire, to lodge within them, and to be fastened to them by screw bolts. Light flat bars *d, d, d*, held by the same screw bolts, were placed between the bars of the spire, to keep them at due distances from each other, thus forming a species of *diaphragm*, fig. 3, where A represents the diaphragm resembling the rudiments of a spider's web, *c c*, &c. the cheeks of each transverse bar of the diaphragm, and *b b b* the bolts, which connect them with the legs of the spire.

Angular braces.

Beside these diametrical supports there are four bars, B B, fig. 6, 20 feet long, placed obliquely from the bottom of one bar to the opposite bar, to which they are connected by screw bolts, thus forming angular braces.

Spindle.

The spindle of the weathercock rises 5 feet above the apex of the spire, and, passing downward through the junction of the bars, it is inserted into a solid diaphragm under and against which it is keyed by a forelock.

Diaphragms.

Beside this diaphragm, and that which forms the base of the spire, there are three others D D D, fig. 6, of a construction similar to that of the lowest diaphragm, placed at equal distances from each other. It is to be observed, that the cheeks or ends of the three upper diaphragms project beyond the upright legs of the spire, to assist in supporting the slates; but the cheeks of the lower diaphragm take in not more than two inches of the feet of the bars of the spire; which feet, as may be seen at fig. 4, are considerably broader than the rest of the bars.

Feet of the bars.

At B, fig. 4, a tenon is formed at the heel of the foot of each bar, which is to receive a key, or forelock, to fasten the spire to the tower, after it has been raised to its place.

Carriage for raising the spire.

To raise and guide this spire, a pedestal, the plan and section of which are seen at fig. 2, and 6, was constructed. It consists of a top and base, each formed of four pieces of deal 6 inches square, and of eight jambs, or uprights, of the same breadth and thickness, and 10 feet high, morticed into the base and top, so as to stand nearly under the eight legs of the spire when it is raised upon it. See fig. 6, where J J J show the position of these uprights. The uprights are strengthened by braces, *o b b o*, so as to prevent them from *racking*, or moving obliquely. The pedestal was furnished with eight wheels 6 inches in diameter, at its upper corners; and with eight similar wheels at its lower corners; as in the plan, fig. 2, and in the section, fig. 6, *w w*.

To facilitate and guide the movement of this pedestal upwards, the tower was lined at each corner with thin planks, P P, fig. 1, fastened to the walls perpendicularly, and adjusted with care. Against these planks the wheels of the pedestal moved upwards with little friction, keeping the spire perpendicular in its ascent.

When this pedestal was adjusted, the skeleton, which had been fitted on the ground, was taken to pieces.

Manner in which the spire was put together.

The base, or lower diaphragm, upon which the bars had been adjusted, was placed and fastened in a temporary manner on the pedestal. The long bars were drawn up, one by one, into the tower above the platform; and their feet were inserted into the cheeks of the base, or lower diaphragm; where they were secured by bolts, as before described. The other diaphragms, and the iron cross braces, were then inserted between the iron bars, and firmly bolted to them.

Covered with slates.

By the favour of Messrs. Worthington and Co. of Penrhyn, I was furnished with excellent slates of dimensions sufficiently large to cover the spaces between the bars, which at the base were nearly 4 feet wide. The slates were 2 feet 6 inches high, and nearly an inch thick*. These slates were sawed to fit upon the ribs where they met, and they were rabbeted with the saw and chissel to lap over each other, so as to keep out water. They were so well joined by these means as to present one even surface, on which the courses of the slates scarcely appeared through the paint. These joints might by addional paint have been entirely concealed, but their appearance was thought to be advantageous, as it gave an idea of solidity, from its nearer resemblance to stone.

Mode of fastening the slates

It remains to show how the slates were fastened to the iron upon which they were placed. For this purpose grooves about one quarter of an inch deep were sawed in the upper surface of each slate, parallel to the bars, and at the distance of nearly two inches from them. A copper capping,

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Best saws for cutting slates.

* The slates were first cut with sand, and such saws as are used for cutting marble. Though this is the method followed at Penrhyn, I found common saws of a smaller size, such as are usually sold for half a crown, far more expeditious.

In cutting the grooves, that receive the copper capping, I employed thin saws with a wooden back, which was held in the hand of the workmen. To make these saws, I cut the blade of small saws into four parts with common tinkers' sheers.

Air holes cut in them.

Air holes in form of a *quatre feuille* were made near the top of the spire, to permit the circulation of air, and they serve also to facilitate the application of a moveable scaffold, whenever the spire requires new painting.

early

nearly semicircular, and about four inches in diameter, was placed so as to cover the joints of the slates, where they met the bars, sinking into the grooves which were just sufficiently wide to receive the copper. The copper by its shape and elasticity *caught* in the grooves, so as to form, when painted, a covering perfectly impervious to rain and snow.

Mode of fastening the slates.

To fasten these copper caps and the slates to the skeleton of the spire, a contrivance was adopted, which requires some detail to become intelligible. The general idea was to fasten the capping and the slates *from within*, so as to leave no holes to be stopped on the outside by putty or paint. Fig. 7 is a section of the slates on a larger scale than that of the spire, where they join on the rib; of the copper capping; and of a collar, or band, by which they are connected with an iron cramp, that passes round the inside of the rib, and, hooking into the collar or band, is wedged within, against the inside of the rib. In looking at this section, care must be taken to distinguish the circular edge of the copper capping from the edge of the band or collar. The band, as may be seen in the drawing, is twice as thick as the capping. In this section of all these parts, as connected together, C is the copper capping; S S, the band or collar; H H, the cramp, or holdfast; and W, the wedge.

The whole of this apparatus for fastening the slates succeeded to my wishes: it was easily executed by common workmen; the parts were easily put together; and, when adapted to their several places, they held the slates and their capping firmly upon the bars, at the same time producing a very good effect by raising a bold and ornamental moulding, or *torus*, fig. 7, on every angle of the spire. It is scarcely necessary to add, that part of the lower corner of each slate was cut away at A to permit the cramps to pass through, and to embrace the iron rib; and that the ends of the diaphragms were permitted to extend beyond the outward surface of the ribs, to support the perpendicular pressure of the slates. Such slates as were not thus supported rested upon the rabbets of those that were beneath them.

The machinery, by which the spire, when it was thus finished, was drawn up, must now be described.

Description of the machinery for raising the spire.

The pedestal.

The plan of the pedestal, the top and bottom of which are similar, is represented at fig. 2, where 1, 2, 3, 4, &c. are the bottoms of the eight jambs, or uprights, of the pedestal; and W W, &c. the wheels, or rollers.

A section of the pedestal, fig. 6, is drawn in the inside of the section of the tower.

b b, cross braces.

The spire.

D D the base, or lower diaphragm, of the spire, resting on the pedestal, to which it is attached by four bolts (of which two only are seen in the section) with forelocks, F F, so as to be easily detached from each other.

L L L L, the legs of the spire.

D D, the diaphragms.

S, the spindle of the weathercock, passing through the apex of the spire.

C, a conical collar, or ring, enclosing the top of the legs. A shoulder is formed on the spindle, and rests on this ring; and as the collar, or ring, projects a little above the tops of the legs of the spire, it could be forced downwards, till the shoulder touches the tops of all the legs, which are cut even, and horizontal at top, so as to permit the collar, the legs of the spire, and the spindle, to be firmly bound together. This is done by means of a mortice, or keyhole, formed in the lower part of the spindle which passes through the small solid diaphragm *d*, against which it is wedged by the forelock *f*.

Method of fastening the spire in its place on the tower.

The heels of all the bars, with the tenon at B, fig. 4, (where it is drawn upon a larger scale) pass through consols, X X, fig. 1, of stone cupped with cast iron, that project from the wall of the tower. The iron cappings of these consols, fig. 8, are made of cast iron; and have apertures left in them, through which the heels of the bars, which form the spire, may pass. When they have all been raised through the consols, eight washers, fig. 9, with a mortice, *m*, in the centre of each of them, are laid upon the consols, and, the spire being allowed to descend, the tenons in the heels of the bars fall into the mortices, and rest upon the consols, and eight other washers are placed upon the tenons, under the consols beneath which they are keyed by forelocks.

T T, fig. 1, the walls of the tower.

W W, the horizontal windlasses; over which two of the ropes were coiled, once round, with weights hung to them.

r r, pullies, over which the ropes passed. Of these there were ten sets, with weights, to counterpoise the pedestal and spire.

h h, handspikes.

The spire drawn up,

Four men were sufficient to work both the windlasses; and on the 19th of this month, before a very respectable concourse of spectators, the spire was drawn up without difficulty or noise in eighteen minutes. It was soon detached from its pedestal, and fixed in its proper place on the consols, with the washers and keys, or forelocks.

A sufficient number of the counterbalancing weights were cut off by sheers; and the men, who had worked the windlasses, descended upon the pedestal to the bottom of the tower.

placed truly perpendicular,

A plumbline was hung from the top of the spire within side, by which it was properly adjusted; and by a few wedges it was placed perfectly upright.

and farther secured.

To add security to the connexion between the spire and the tower, iron cramps of 7 or 8 feet long were hooked into the mortices, which had served to join the legs of the spire to the pedestal, and were firmly fastened to the walls of the tower by proper holdfasts: so that, though the spire and tower may be blown down together, it is scarcely possible, that they can be severed by the violence of any storm.

Expense of the spire.

The cost of this spire has not yet been entirely ascertained, but it does not exceed one hundred and fifty guineas. A spire of the same dimensions, built of Portland stone, would, in this country, cost at least six times this sum, and if it were formed of the limestone of the country, it would cost four or five hundred pounds.

I was this day, September the 22nd, enabled to determine, whether strong wind had any sensible effect on the spire, as its spindle happens to coincide with a vertical wire of a transit instrument in my observatory. The violence of a sudden squall did not seem in the least to affect it.

I have therefore reason to hope, that it will remain undisturbed by future storms: and, as a thunderstorm passed over this place the night before, I trust, that the conductor, which has been attached to the iron legs, will secure the spire from the effects of lightning.

I am, Sir,
Your obedient servant,
RICHARD LOVELL EDGEWORTH.

It has occurred to me since the spire was finished, that, instead of a temporary wooden pedestal, an iron permanent pedestal might be substituted, which might be formed by a continuation of the legs of the spire. At the base of this pedestal, if it were thought necessary, a brick arch might be turned on the lowest diaphragm. This would add weight, and consequently solidity to the mass. This pedestal must be connected with the tower by holdfasts and wedges.

I mention this, not because I find any inconvenience in what I have executed, but to communicate to the public all that has occurred to me on this subject.

II.

Experiments on some Preparations of Gold: by Mr. VAUQUELIN*.

Preparations of gold employed medicinally.

SINCE Dr. Chrestien, of Montpellier, mentioned the effects he had obtained from the use of preparations of gold in syphilitic and lymphatic complaints; and remarked, that these effects were never attended with the ill consequences, to which mercurial preparations often give rise, other physicians have begun to make use of them.

The forms in which gold has hitherto been employed are, 1, in a state of minute division: 2, the muriate: 3, the oxide precipitated from a solution of gold by potash: 4, the precipitate thrown down by metallic tin from the muriatic solution of gold.

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* Annal. de Chim. vol. LXXVII, p. 321.

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Difficult to obtain these uniform.

There is some difficulty in obtaining these preparations constantly in the same state; and one of the principal objects in the art of physic being precisely this constancy in the nature of medicines, it appeared to me of some utility to examine these preparations, and to describe with accuracy the processes best adapted for obtaining them.

SECT. I. *Of the quality and quantity of nitromuriatic acid most suitable for dissolving Gold.*

Nitromuriatic acid. 2 p. muriatic, 1 p. nitric, dissolve 1 p. of gold.

It was formerly the practice, to compose nitromuriatic acid of two parts of nitric and one of muriatic, by weight. But on considering, that gold requires only a very small portion of oxigen for its solution, and that the nitric acid in the process in question answers this purpose alone, I concluded, that the same purpose would be obtained, if an aqua regia were composed of the two acids in opposite proportions to those hitherto directed. In fact, three parts of nitromuriatic acid thus made were sufficient to dissolve one part of fine gold, while at least four made in the old way were required.

But little oxigen combines with gold.

A proof of the small quantity of oxigen, that combines with gold at the moment of its solution may be found in the very small quantity of nitrous gas evolved: beside which there is reason to presume, that some portion of this gas is produced by the action that takes place between the two acids, since some oximuriatic acid is evolved likewise

The solution crystallizes,

The solution of gold, when duly evaporated, crystallizes in yellow prisms, the figure of which, I believe, has never yet been ascertained with precision.

but is partly decomposed without great care.

The evaporation of the solution must be conducted with great caution, otherwise part of the salt will be decomposed, and the gold will reappear in its natural state, in the form of small scales.

Action of alkalis on it.

The solution of muriate of gold comports itself with the fixed alkalis in a manner different from that of other metals; most of which, it is well known, are completely precipitated by them in the state of oxide.

Do not precipitate it,

Potash, soda, barytes, and lime, do not render the solution of gold in the least turbid, at common temperatures. It only acquires a very deep red colour with potash and soda nearly like that of Stahl's alkaline martial tincture. No change in the limpidity of these mixtures takes place on standing.

Barytes and lime do not produce the same colour in the solution of gold, no doubt on account of the great quantity of water employed in their solution.

[illegible] assisted by heat.

If, after the acid of the solution of gold has been completely saturated by potash, the mixture be heated, a red substance separates in a very bulky flocculent form, much resembling in appearance oxide of iron at a maximum.

Precipitate with excess of alkali.

If an excess of caustic alkali, even though very trifling, be put into the mixture, and it be boiled, the bulk of the precipitate will diminish greatly, and it will appear of a brown colour, when seen in a body; though it is in reality blue, for the particles of matter suspended in the liquor, *which of itself is slightly yellow, make it appear green**.

The menstruum still retained some gold.

The liquid, from which I had precipitated the matter abovementioned by means of potash, was colourless; but, as soon as it was saturated with muriatic acid, it suddenly assumed a yellow hue, like that of the common solution of gold, and sulphate of iron threw down metallic gold from it.

The precipitate slightly soluble.

All the washings of the precipitate, to the last, gave signs of the presence of gold; which seems to indicate, that this matter is slightly soluble in water. The last washings however contained less than the first.

Action of sulphate of iron on solutions in different proportions.

When the liquors contain a certain quantity of gold, the precipitate formed in them by sulphate of iron presently assumed a brown colour; but when they contain only a little of this metal, no precipitate is formed immediately, the liquid only becoming of a fine transparent indigo blue. At length however, a black powder is deposited, leaving the liquid colourless.

Colour of gold.

This observation seems to prove, that when gold is in a state of minute division, it appears blue; and that it assumes its natural colour only from the union of a certain number of its particles.

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* Two things are here taken for granted; that the precipitate is homogeneous, and that the suspended particles are precisely the same with it. From the next paragraph too it would appear, that the "slightly yellow" liquid is colourless. C.

This would explain, 1st, why a very thin leaf of gold, perforated with minute holes, when held between the eye and the light appears green; because the blue colour of the most minutely divided particles mixes with the yellow of those that are less so: 2dly, why, when to a somewhat concentrated solution of gold sulphate of iron is added in sufficient quantity to reduce the whole of the gold, the liquid is of a fine green; because the yellow colour of the particles of gold united in little masses combines in some measure with the blue of those that are not yet united: and 3dly, why, in proportion as the former fall down, the liquid gradually changes to a pure blue, which it continues till the whole is precipitated. Hence it is probable, that the precipitate of Cassius does not consist wholly of metallic gold, but is rather a mixture of oxide of gold, oxide of tin, and a little metallic gold.

Purple powder of Cassius.

Action of carbonate of potash on the solution.

Carbonate of potash also added to a solution of gold does not effect its precipitation, but only produces an effervescence. At the expiration of thirty hours the solution becomes turbid, without any thing separating; and it assumes a very rich red colour, in proportion as the carbonic acid it had absorbed flies off.

Precipitate on boiling.

On boiling this mixture a very thick magma is formed of the colour of pale kermes mineral; but this colour is not altered by ebullition with excess of carbonate, as is the case with caustic potash, which indicates, that the latter has some action on the precipitate.

Gold still in the liquid.

When the liquid, from which the red matter was separated, appeared to have lost its colour, I filtered it, to obtain the precipitate by itself. The liquid then exhibited only a very slight tint of yellow, whence, and from its taste, which was by no means metallic but simply saline, it might have been presumed no longer to contain any gold; but this would have been a mistake. In fact a part of the liquid, into which I let fall a few drops of muriatic acid, immediately assumed a very decidedly yellow colour; and on the addition of sulphate of iron it threw down a pretty considerable quantity of metallic gold.

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The examination of this liquid I deferred, till another time, to attend to the red precipitate formed by the carbonate of potash in the solution of gold.

The precipitate examined.

Slightly soluble in water.

Dried.

Its colour.

Only two thirds of the gold precipitated.

I began by washing this substance with boiling water, taking care to keep each of my washings separate, that I might more easily satisfy myself when it no longer contained any thing soluble: but though I thus used a very large quantity of water in proportion to its bulk, I was never able to exhaust it; and it appeared to me, that the last washings contained nearly as much gold as the first. Hence I was led to suspect, that the precipitate was slightly soluble in water, and that, by continuing to wash it, I should perhaps cause it to disappear entirely. In consequence I ceased washing the precipitate, and dried it slowly. It greatly diminished in bulk, which proved, that it contained a large quantity of water. Its colour became a great deal deeper, and resembled that of dried blood; but when powdered it was of an orange yellow. 7·643 gram. [118 grs] of fine gold, precipitated as mentioned above, furnished only 5·414 gr. [83.7 grs] of red matter: whence it follows, that 2·229 gr. [34·3 grs] of gold at least, or a little less than a third, remained in the mother-waters, and in the washings.

No excess of carbonate in it when washed.

Though I employed an excess of carbonate of potash to precipitate the solution of gold, the red matter I obtained did not contain any sensible quantity of this salt: for after it was dried, it dissolved entirely in muriatic acid without producing the least effervescence; which proves, that it had been entirely divested of carbonate by the washings, and that the precipitate it formed retained no carbonic acid.

But it retained some muriatic acid.

But it was not the same with respect to muriatic acid; for it was necessary to employ repeated portions of nitric acid, as will be seen below, to deprive the precipitate completely of the muriatic: after this the nitric solution no longer afforded a precipitate with the nitrate of silver.

Probably an oxide of gold with a little muriate.

The presence of muriatic acid in the first solutions of this matter in nitric acid led me to suspect, that it was in the state of muriate of gold with excess of oxide; but as the latter contained no more of this acid, it appeared to me more probable, that it is simply an oxide retaining a few atoms of muriate, notwithstanding the repeated washings it had undergone.

Why is not the precipitate homogeneous?

But if potash and its carbonate precipitate in the state of oxide part of the gold dissolved in muriatic acid, why do they not precipitate the whole? and what becomes of the part left in the liquid, and in what state is it there?

This we shall examine by and by: at present let us describe the properties of oxide of gold.

Properties of the oxide of gold thus obtained.

The oxide of gold, prepared in the manner above-mentioned, has very sensibly a styptic metallic taste, which excites the secretion of saliva copiously, and for a long time. If it be diluted with water, and blotting paper, or any other porous combustible substance, be impregnated with it, it causes them to burn with scintillation, as gunpowder would do. A decigramme [1·544 gr.] of this oxide, in a state of minute division, and shaken for some time in 60 gr. [926·7 grs] of distilled water, was not dissolved, at least entirely: the filtered liquor however, though perfectly clear and colourless, afforded with sulphate of iron a pretty copious blueish precipitate, which was metallic gold. This proves, that a solution in water had taken place: but as this solution might have arisen from some portions of salt remaining with the oxide for want of sufficient washing, I poured fresh portions of water repeatedly on the undissolved portion, and by the same means as above-mentioned found gold dissolved in them all; though it is true the proportion gradually diminished as the washings were more numerous. Though I did not dissolve the decigramme of this substance entirely, apparently because the latter portions were not sufficiently divided; I have no doubt from the little that remained, that I should at length have dissolved the whole, if I had continued my trials.

What seems to prove it is, that the last washings, which still gave evident signs of the presence of gold, when tested with sulphate of iron, afforded no appearance of the presence of muriatic acid on adding nitrate of silver.

Alkalis precipitate gold as an oxide.

From these experiments we may presume, that potash, soda, and their carbonates, precipitate gold from its solution in the state of oxide; or that, at least, if any muriatic acid remain in the precipitate, it must be an infinitely small quantity, when the washings have been conducted with due care.

Its medicinal qualities.

The slight solubility of this oxide, and its very easy decomposition, must render its action, as an oxigenizing substance, in the animal economy, prompt and certain.

Similar to those of the red oxide of mercury.

The red oxide of mercury, which has some properties in common with the oxide of gold, namely those of dissolving in water and of being easily decomposed, possesses nearly similar medicinal virtues; and from analogy we may conjecture, that oxide of silver also would have the same properties.

Oxide of silver probably analogous.

Action of nitric acid on the oxide.

Nitric acid does not attack dry oxide of gold, unless it be employed in large quantity, and in a concentrated state. In this it differs greatly from the muriatic acid, which dissolves it immediately. The nitric solution of gold has a brown hue; and water throws down from it a flocculent precipitate, of the same colour as that occasioned by alkalis.

The first portions of nitric acid, that have been decanted off the same oxide of gold, form a precipitate with the solution of silver, after the gold has been thrown down from them by water; but the latter portions are not precipitated, which confirms what has been said above.

The affinity of the oxide of gold for nitric acid appears very weak, for part separates in the metallic state by spontaneous evaporation. This no doubt is the reason why nitric acid alone cannot dissolve this metal.

SECT. II. *Examination of the liquor, from which gold has been precipitated by fixed alkalis.*

The liquid, from which gold had been precipitated, examined.

I have said, that this liquor has no perceptible colour, but that it resumes a pretty deep yellow, when muriatic acid is added, and that afterward an addition of sulphate of iron throws down metallic gold from it pretty copiously.

Muriate of potash first separated, then carbonate,

Having evaporated this liquor by a very gentle heat, I obtained at first crystals of muriate of potash; among which were observable some other crystals of carbonate of potash, this salt having been added in excess. The liquor being decanted from these salts, and evaporated anew with the same precautions, acquired a slight yellow tinge, and at length furnished a salt of the same colour, which had no regular figure. With this were mixed a few crystals of carbonate of potash perfectly colourless. The coloured salt, being well drained, produced no very decisive effervescence with muriatic acid, though the colourless crystals effervesced with it briskly; but its solution was not coloured. The coloured crystals, when redissolved in water, yielded a copious precipitate of metallic gold on the addition of sulphate of iron. The mother-water of these crystals effervesced with muriatic acid, and afterward gave a precipitate of metallic gold with sulphate of iron.

and lastly a yellow salt;

These experiments seem to prove, that these crystals, as well as their mother-water, are composed of muriate of gold and muriate of potash united together in the state of a triple salt; and that the carbonate of potash is only mixed with them.

being a triple muriate of gold and potash.

Hence it appears very probable, that, if a solution of gold, as nearly in the neutral state as possible, were mixed with a sufficient quantity of muriate of potash, alkalis would throw down no precipitate from this mixture.

Perhaps this salt not precipitable by alkalis:

To prove this, I made the experiment above; but I obtained a precipitate with carbonate of potash: though it is true much less abundant, of a different colour, and of a different appearance, from that obtained with a solution of pure gold. Its colour was yellow, and its form granular, not flocculent like that of oxide of gold.

but the conjecture not confirmed by experiment.

An examination of this precipitate informed me, that it was composed of muriate of gold, and muriate of potash rendered little soluble by the presence of alkali in the liquor, from which it had been separated.

Precipitate.

One thing remarkable is, that, after having precipitated a solution of gold by means of an excess of saturated carbonate of potash, if a sufficient quantity of acid to decompose the alkaline salt be added to the filtered liquor, a few flocks of oxide of gold will be separated; and afterward, this liquor being filtered, if muriatic acid be added, it will yield a fresh precipitate by the help of boiling; but the last is a triple salt, similar to that which has just been mentioned.

Oxide of gold thrown down by an acid.

Probably potash holds some in solution

. I think the precipitate formed by an acid in the solution of gold is to be ascribed to a small quantity of this metal held in solution by carbonate of potash. This effect takes place in a still more remarkable manner with caustic potash.

Method of obtaining the largest quantity of precipitate.

From what has been said it is evident, that, to precipitate the greatest quantity of oxide of gold possible from its muriatic solution by means of alkalis, we must manage so, that no useless acid remains in the solution; in order that less of the triple salt may be formed, on which the alkalis have no action. This is effected by evaporations to dryness very cautiously conducted.

The liquor from which gold has been precipitated should not be thrown away.

Much thus lost by jewellers.

It follows too from what has been said, that the liquors, from which gold has been precipitated by alkalis, should not be thrown away, for they still contain a considerable quantity of the metal. On this occasion I may relate a curious anecdote, which shows, that many things are lost sometimes in the arts, and in manufactures, from which advantage might be derived, if we had the requisite knowledge. For many centuries jewellers had been accustomed to throw away as useless the waters, with which they cleaned their work, and thus at least two or three thousand francs were annually lost in Paris alone. But since I taught them, that these waters contained gold, and showed them the mode of getting it, they preserve them carefully.

I am at present busy in examining the nature of the gold precipitated from its solution by metallic tin, which is also employed as a medicine; and as soon as I have finished my investigation I shall lay the result before the Society [of Pharmacy at Paris].

III.

Experiments on Human Bones, as a Supplement to the Paper on the Bones of the Ox: by Messrs. FOURCROY *and* VAUQUELIN[*].

Magnesia supposed to exist

WHEN in the month of August, 1803, we published our first paper on the existence of magnesia in bones, we in the bones of quadrupeds only.

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[*] Journal de Physique, vol. LXX, p. 185.

announced, that we had not found any in human bones; and thought we might presume the cause of this difference to be the excretion of phosphate of magnesia by the urinary passages in man, while none occurs in the urine of animals.

However, as we had made only a single experiment in search of this substance, we did not assert positively * the absence of magnesian earth in these organs.

Human bones more strictly examined,

On occasion of our last publication, in the month of September, 1808, on the presence of iron and manganese in ox-bones, we thought it necessary to resume with great care the analysis of human bones, not only with respect to magnesia, but also of the metals in question.

yielded magnesia, iron, and manganese:

In treating these bones in the manner we have mentioned with respect to those of the ox †, we found in them magnesia, iron, and manganese, in the same state as in the latter.

but less of the first, and more of the other two, than those of quadrupeds

If we may be allowed to reckon on the proportions of the substances we obtained from human bones, they appeared to us to contain less magnesia, and more iron and manganese, than the bones of herbivorous quadrupeds. The small quantity of the first of these salts agrees with the continual discharge of phosphate of magnesia in the human urine. It is well known, that this is not the case with the urine of herbivorous animals: on the other hand, the iron and manganese, once entered into the course of the circulation, and deposited in the various organs of the animal economy, no longer finding an exit from the body, the quantity of these two substances apparently must increase with age, and from the known nature of food; so that the blood and bones of an old man ought to contain more iron and manganese than those of children, as well as of animals, who besides do not live so long as man. Thus the proportions with respect to quantity confirmed by our experiments are equally so by known physiological phenomena.

They contain also alumine and silex.

Our last researches have shown us traces of alumine and silex likewise in human bones. The last exists in the phos-

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* It appears however to be asserted positively enough in the paper referred to. See Journal, vol. VIII, p 86. C.

† See Journal, as above quoted. C

phate of ammonia resulting from the precipitation of phosphate of magnesia by volatile alkali. On evaporating to dryness, and slightly calcining the residuum, this earth is obtained of a black colour, and in a flocculent form; but by calcination at a red heat it assumes all its characteristics.

We suspected at first, that the silex and alumine might have been taken up by the phosphoric acid from the stone vessels we used: but we have since satisfied ourselves, by several decisive experiments, that they actually existed in the bones.

Though we have already given an account of the successive operations necessary for obtaining the different substances just mentioned, in the *Annales du Muséum d'Histoire naturelle* for September, 1808, we shall repeat them here, to form a complete whole, and as a guide to those who would go through the same examination.

Method of analysis.

1. Let the bones, calcined and powdered, be decomposed by an equal quantity of sulphuric acid.

2. Dilute the first mixture with twelve parts of distilled water: pour the whole on a piece of cloth, leave the sulphate of lime to drain, and wring it out strongly.

3. Filter the liquor through paper, and precipitate it by ammonia; filter it a second time, wash the precipitate, and set the liquor aside.

4. While the precipitate is still wet, treat it with sulphuric acid, taking care that the acid is a little in excess: filter afresh, wash the precipitate, and add the liquor to the former: No. 3. Repeat this operation, till the precipitate formed by the ammonia dissolves entirely in the sulphuric acid; which will show, that it no longer contains any sensible quantity of lime.

By this series of operations the whole of the lime in the bones will be converted into sulphate of lime, which, being but little soluble, will be separated from the liquor; in which will be found the phosphoric acid, with the sulphates of magnesia, iron, manganese, and alumine.

5. These substances, being separated from the sulphuric acid by ammonia, are to be treated with caustic potash, which will attract the sulphuric and phosphoric acids, evolve the ammonia, and dissolve the alumine.

6. Precipitate the alumine from the alkaline solution by

means of muriate of ammonia, wash it, and examine by the usual means whether it be really alumine.

Method of analysis.

7. Dry the magnesia, iron, and manganese, from which the phosphoric acid and alumine have been separated by the potash. Calcine them a long time in a platina crucible, and pour on them sulphuric acid diluted with water, till there is a slight excess of it.

This will dissolve the magnesia, and a portion of the iron, but not touch the manganese.

8. Evaporate the solution of magnesia containing iron, and calcine it strongly: the iron will be separated, and the magnesia, on the contrary, will remain united with the sulphuric acid. Dissolve in water, and the iron will be obtained in the state of red oxide. Precipitate the magnesia by carbonate of potash, and ascertain its purity by the usual methods.

9. Add the iron of the preceding operation to the manganese of experiment 7, and dissolve them both in an excess of muriatic acid. Dilute the solution with water, and add carbonate of potash, till a red flocculent precipitate sepaparates, and the liquid becomes clear and colourless.

These flocks are oxide of iron. Let them be separated by filtration, and boil the liquor in a matrass. After some time, the manganese will fall down in a white powder, and when the liquor lets fall nothing more, and potash produces no effect on it, separate the manganese by filtration. Calcine it, and it will become black.

Thus the alumine, magnesia, iron, and manganese, having been separated by the means just described, nothing remains to be done but to find the silex.

10. For this purpose evaporate the liquor containing the phosphate and sulphate of ammonia of experiments 3 and 4. As it concentrates, tolerably bulky black flocks are formed, which must be separated from time to time by filtration; and when the salt is thoroughly dry, it is to be dissolved in water, and a little more of the same black matter will be obtained.

11. Wash this flocculent matter, calcine it in a platina crucible, and a white powder will be obtained, possessing all the properties of silex.

During these operations the ammonia is for the most part extricated, as well as the sulphuric acid, in the state of sulphite of ammonia, and the phosphoric acid is left tolerably pure. Caustic potash however still evolves a little ammonia.

Substances found.

Thus, beside the phosphate of lime, there are in human bones, as well as in those of animals, phosphates of magnesia, iron, manganese, silex, and alumine. The last is in very small quantity; yet enough for its presence to be fully recognized and established.

These vary in their proportions.

It may be supposed, that in this method of analysis human bones will exhibit some variation in the proportions of the substances, according to the age, constitution, state of health, and general difference of the persons to whom they belonged.

The analysis very nice and difficult.

It is equally essential to observe, that, though this analysis exhibits a set of experiments simple enough in their description, it must be reckoned among the most delicate and difficult analyses, on account of the number of successive operations it includes, and the precision it requires.

IV.

Letter from Mr. Berzelius *to Mr.* Berthollet *on the Analysis of different Salts**.

Two propositions of considerable importance to the theory of affinities.

In studying Mr Richter's work, "On modern Subjects of Chemistry", Part I—X, 1795—1800, I found in it two propositions, which appear to me of great importance to the theory of affinities. These are: 1. That all neutral salts, which remain neutral when their solutions are mixed, are so composed, that the quantities of the different bases, that saturate one of the acids present in the mixture, follow the same proportions in saturating the other acids: 2. That a metallic neutral salt, the metal of which is precipitated by another more combustible metal, changes its metal only:

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* Annales de Chim. vol. LXXVII, p. 63.

while the portion of oxigen, that enters into the metallic oxide, and the acid, with which it is saturated, continue the same: and that the different metallic oxides, which saturate a given portion of any acid, all contain the same quantity of oxigen.

The first applied to various analyses.

The first of these propositions appeared to me the most important. The experiments of Mr. Richter being for the most part defective, I began by applying this principle to a great number of other analyses made by different chemists; but among these I found only six, that answered to the rule with any degree of accuracy. These were the analyses of the sulphates and muriates of barytes, potash, and soda, made by Messrs. Bucholz and Rose. The analyses of Mr. Kirwan corresponded very well with each other, but not with other analyses. The experiments I have mentioned of Messrs. Bucholz and Rose, having afforded results differing only in the thousandth parts, appeared to be the most accurate; and almost the only ones, that were sufficiently precise for inquiries of this kind. To determine this point, and in order to verify the opinion of Mr. Richter in a more decisive manner, I proposed to myself to execute a series of analyses with the most scrupulous exactitude; and for this purpose to analyse all the sulphates, and all the salts with base of barytes. From these two sets of analyses I could calculate the composition of all the other salts, and the result of this calculation remained to be confirmed by experiment. I had engaged in this pursuit in 1807, and given an account of some of the analyses in my "Elementary Treatise on Chemistry", which was published in the beginning of 1808. The truth of the principle being fully confirmed by these analyses, nothing remained, but to complete the two sets of analyses I had proposed to myself.

Two sets of analyses undertaken to verify the principle.

Dr. Davy's discovery. All salifiable bases supposed to be metallic oxides.

At this juncture the discoveries of Mr. Davy on the decomposition of the fixed alkalis were published. The idea, that all salifiable bases were metallic oxides, at once struck me; and I had no doubt, that I should soon hear of Mr. Davy's having metallized also the earths and ammonia. I repeated, however, with Dr. Pontin, physician to the king, the experiments of Mr. Davy: but, as we had only a very

feeble

Attempt to form amalgam of ammonia

Attempt to ascertain the quantity of oxigen in ammonia.

feeble voltaic pile, we attempted by means of a metallic conductor, fastened to the negative pole, and immersed in mercury, to collect the small portion of metallic base, that appeared to be formed. The potassium was readily deposited in it, and the little globule of mercury was reduced to a solid amalgam. We repeated the same experiment with ammonia, which was decomposed still more readily. The mercury adhering to the end of the negative conductor yielded a metallic vegetation, resembling that which is formed when a salt with base of lead is decomposed by the operation of the pile. The vegetation increased so considerably in bulk, that at length it separated from the conductor, and, floating on the liquid, was converted into ammonia with effervescence, and evolution of heat. All my endeavours to obtain this substance separate have hitherto been vain. At first I considered it as a metal composed of hidrogen and nitrogen; but the experiments of Messrs. A. Berthollet, Davy, and Henry, with which I have since become acquainted, convince me, that this opinion was unfounded. Being unable to produce this problematic substance without the assistance of mercury, I was desirous at least of ascertaining the quantity of oxigen, with which it is combined in ammonia; and perceiving the impossibility of doing it by direct experiments, I had recourse to the principle of Mr. Richter: that all bases, which saturate the same quantity of any acid, must contain the same portion of oxigen.

Muriatic acid saturated with different oxides.

I weighed with accuracy portions of the amalgams* of potassium, sodium, and calcium; I dissolved the metalloid in muriatic acid, evaporated the solution, and fused the salt in a small gold crucible. Thus I obtained results, that agreed very well with this principle. I had calculated the quantity of base in the salts from the analyses of the muriate of silver made by Messrs. Bucholz and Rose. It appeared, that 100 parts of muriatic acid saturated a quantity of potash, soda, lime, oxide of mercury, and oxide of silver, containing 42 parts of oxigen. In consequence I analysed the oxides of copper, lead, iron, and zinc; and, on combining them with muriatic acid, I believed I obtained the same results; but, after a number of tolerably accuanalyses, my expectation was so disappointed, that I found myself obliged to give up that principle; though, the more I reflected on it, the more probable it appeared. During my analyses of these metallic oxides I had observed another circumstance that caught my attention, namely, that the quantity of oxigen which saturated 100 parts of metal in the oxidule, was increased to half as much more, or double as much in the oxide. Thus 100 parts of lead with 7·8 of oxigen form the yellow oxidule, with 11·7 red oxide, and with 15·6 brown oxide: 100 parts of copper with 12·5 of oxigen form the red oxidule, with 25 the black oxide: &c.

Ratios of the oxigen in different oxides of the same metal.

I then proposed to determine the quantity of oxigen in sulphuric and in sulphurous acid. To remove all moisture from the sulphur, I combined it with lead. I found on this occasion, that lead absorbs precisely twice as much sulphur as oxigen at its minimum of oxidation; and I soon ascertained, that it was the same with iron, copper, and tin. I am since persuaded, that the native sulphuret of iron (the *maximum*) contains for every hundred parts of iron double the quantity of sulphur that exists in the artificial (the *minimum*, magnetic iron ore). From these circumstances sulphur appears to me to follow the same laws in its combination as oxigen. It follows too, that, the composition of an oxide being known, that of the sulphuret is easily found by a simple calculation, and the contrary.

Sulphur follows the same analogy in its combination with metals.

The sulphuret of lead, oxided by nitromuriatic acid, produced a neutral salt, without either the oxide of lead or sulphuric acid predominating. 100 parts of lead combined with 15·6 of sulphur yielded precisely the same quantity of sulphate as 100 parts of lead dissolved in nitric acid, the solution being afterward mixed with sulphuric acid, evaporated to dryness, and the residuum heated redhot. From these experiments I was persuaded, that the sulphuret of lead contains precisely the quantity of sulphur necessary for the formation of the sulphuric acid required to saturate the oxide of lead yielded by the same quantity of sulphuret. Experiments on the sulphuret of iron at a *minimum*, and on the sulphate of oxidule of iron, convinced me, that the same thing took place with the sulphuret of iron.

The sulphur in a sulphuret exactly sufficient to form a sulphate.

From

General laws.

From all this I deduced the following consequences: *a.* A metal combines with sulphur at a *minimum* in such a proportion, that, the sulphur being acidified, and the metal oxidulated, the result is a neutral sulphate of the oxidule: *b.* A sulphate of an oxidule contains half as much oxigen as there is sulphur in the sulphuric acid, with which it is saturated.

Composition of sulphuric acid and sulphates.

Sulphate of barytes.

Muriate of silver.

From repeated experiments I have found, that sulphuric acid is composed of 40 parts sulphur, and 60 oxigen, almost precisely; and that 100 parts of sulphuric acid saturate a quantity of base containing 20 parts of oxigen. The following is an incontestible proof of the truth of this opinion, which I was on the point of giving up. On comparing the result of my experiments with that of the experiments of Mr. Bucholz, who had found 42 parts of sulphur and 58 of oxigen in sulphuric acid, I discovered, that his analysis of sulphate of barytes was inaccurate. According to him this salt is composed of 32·5 acid and 67·5 base. I find it to consist of 34 acid and 66 base*. The inaccuracy of the analysis of the sulphate occasioned an inaccuracy in the analysis of the muriate of barytes, and in that of the muriate of silver. I endeavoured to correct these defects by experiments as accurate as possible, and found the muriate of silver to be composed of 18·7 muriatic acid, and 81·3 oxide of silver. On applying these corrections to my former analyses I perceived the harmony, that I had hitherto missed. Every thing then confirmed me in the opinion, that the different bases, which saturate the same quantity of any acid, contain the same quantity of oxigen.

Sulphurous acid

On oxidating sulphite of barytes by means of nitric acid I obtained neutral sulphate of barytes, without any superfluous sulphuric acid, or nitrate of barytes, being formed. The increase of weight of the sulphite taught me, that sulphurous acid consists of almost exactly equal parts of sulphur and oxigen; or, that 100 parts of sulphur combine with near 100 parts of oxigen to form sulphurous acid, and with about 150 to form sulphuric acid. From these experiments

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* For analysis of the sulphate of barytes by Mr. James Thomson and Mr Berthier, see Journal, vol. XXIII, p. 174, and 280.

riments I conclude, that sulphurous acid presupposes in the bases with which it is saturated the same quantity of oxigen as sulphuric acid. It appears to me probable also, that the metal and sulphur always remain in the same proportion to each other in the sulphuret, sulphuretted oxide, sulphite of the oxidule, sulphate of the oxidule, and combination with sulphuretted hidrogen. But I have proved, that the proportion between the metal and sulphur is altered in the sulphates of the oxides, when the oxigen in the oxide is equal to that of the oxidule multiplied by 1·5.

Examination of muriates,

By the analysis of the muriate of lead I found, that the base, which saturates 100 parts of muriatic acid, contains 30·49 parts of oxigen: and on calculating from this result the composition of the oxidule and oxide of copper, of the oxides of silver and lead, and of potash, soda, and lime, I always obtained results agreeing sufficiently with those of the direct experiments. The sulphates of iron, copper, lead, lime, potash, and soda, giving also, both by calculation and experiment, results corresponding with each other and with those of the muriates, I have imagined, that this point may be considered as completely settled. It is to be understood, that all these different analyses could not be carried to such perfection, as to give results not varying in the thousandths, and sometimes even in the hundredth parts; but these circumstances are to be ascribed rather to the difficulty of executing analyses with perfect accuracy, than to an erroneous principle.

and of sulphates, confirms the principle.

Oximuriatic acid.

The oximuriatic acid combines with metals, and forms neutral salts, in which neither the acid nor oxide predominates. Hence 100 parts of muriatic acid are combined with the same quantity of oxigen in the oximuriatic acid as in the muriatic salts, that is to say, with 30·49 parts. In the experiments of Mr. Davy, potassium exposed to common muriatic acid gas was condensed, forming a neutral salt, and evolving hidrogen gas. It is evident therefore, that 100 parts of muriatic acid are combined with a quantity of water, that contains 30.49 of oxigen; that is, with 34·5 of water. Concentrated sulphuric acid contains, according to accurate experiments, almost a fifth part of water: that is to say, 100 parts of this acid are combined with 22·6 of

Muriatic acid gas

combined with water

Sulphuric acid.

water,

All substances, that combine with a given acid, contain a given quantity of oxigen.

water, which contain 20 parts of oxigen. It appears, then, that this rule may be applied to every other substance, mineral, vegetable, or animal, which forms with acids a marked or neutral compound; for instance, the matter of the bile, albumen, and several colouring matters. In short, this law may be extended to all the acids, and every substance in any way capable of saturating them.

Composition of water.

To ascertain the composition of water I employed distilled zinc and sulphuric acid. The decomposition was performed in an apparatus accurately weighed; and the hidrogen gas was transmitted through a tube filled with muriate of lime. 200 parts of zinc yielded 248·8 of oxide, and evolved 6·5 of hidrogen gas. According to this experiment water is composed of 11·75 hidrogen and 88·25 oxigen; which agrees exactly with the experiments of Messrs. Biot and Arago. On dissolving a quantity of sulphuret of iron at a minimum in muriatic acid, I received the sulphuretted hidrogen gas in a caustic lixivium, by which it was entirely absorbed. Hence it follows, that the sulphur, which saturates 100 parts of hidrogen, must be to the oxigen, that saturates the same portion, in the same ratio as the sulphur is to the oxigen with which 100 parts of iron are saturated. The quantity of oxigen that saturates 100 parts of hidrogen being 750·77, the quantity of sulphur must be 1501·54, and sulphuretted hidrogen gas is composed of 6·243 hidrogen and 93·756 sulphur.

Sulphuretted hidrogen gas.

Composition of ammonia.

After all these experiments I thought, that a calculation of the composition of ammonia might afford a result, that would at least approach the truth. Accordingly I analysed the muriate of ammonia, and found it to be composed of 49·46 muriatic acid, 31·95* ammonia, and 18·59 water of crystallization. Consequently 100 parts of acid are saturated by 64·6 of ammonia. From analogy with the other alkalis this quantity must contain 30·49 of oxigen; and hence it follows, that ammonia is composed of 47·2 oxigen and 52·8 metallic base.

Quantity of oxigen in an acid may be deducted from that in the

It was to be presumed, that a salifiable base would determine in some measure the quantity of oxigen in the acid required for its saturation; but this proportion was not so easy to find as that between the acid and oxigen in the base.

base which neutralizes it.

I was fortunate enough however to discover it. It is as follows:

Law of the proportion.

"In a compound formed by two oxided substances, that which, in the circuit of the electrical pile, ranges itself round the positive pole (the acid, for example) contains two, three, four, five, &c. times as much oxigen as that which ranges itself round the negative pole (for example, the alkali, earth, metallic oxide)." This law, being applicable to many other combinations beside salts, will soon impart to chemical analysis an unexpected degree of perfection. Most acids contain twice as much oxigen, as the bases that saturate them, as the carbonic acid; others three times as much, as the sulphuric acid for instance; and others, as the hyperoximuriatic acid, as far as twice* as much. In all these compounds water acts an important part; sometimes we find it uniting as a base with the acids, for instance with the crystallized vegetable acids and mineral acids; and at other times taking the place of an acid, and combining with the alkalis, earths, and metallic oxides, forming what we called hydrates.

Compounds of the muriatic base with oxigen.

There is every appearance, that the muriatic acid contains twice as much oxigen as the bases that saturate it. In this case it is composed of 61·3 oxigen, and 38·6 base; or 100 parts of the base combine with 156 of oxigen to form common muriatic acid, with 234 to form oximuriatic, and with 624 to form the hyperoximuriatic.

Bulks of gasses that enter into combination.

It is but very lately, that I have had an opportunity of reading the interesting work of Mr. Gay-Lussac on the bulks of the gasses that enter into combination. It is evident, that his experiments confirm a part of the ideas, which I have had the honour to communicate to you. They contain facts, of which I have availed myself, to acquire information on a subject, the knowledge of which it was highly important to me to obtain. According to Mr. Gay-Lussac,

Compounds of carbon and oxigen.

100 cubic inches of carbonic oxide gas mixed with 50 cubic inches of oxigen gas produce 100 cubic inches of carbonic

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* In the original 2 *fois*, but the figure is palpably erroneous from the context. From the succeeding paragraph it should probably be 8 *fois*; eight times instead of twice. C.

acid gas. Consequently carbon combines with oxigen in two proportions, one of which is double the other: and as 100 parts of carbon are combined with 251·637 of oxigen in carbonic acid, they absorb 125·818 to form carbonic oxide gas. Dr. Thomson in his analysis of inflammable gasses has given the following particulars respecting carburetted hidrogen gas. 100 cubic inches of carburetted hidrogen gas consume 200 c. i. of oxigen gas, and form 100 c. i. of carbonic acid gas: 100 c. i. of olefiant gas consume 300 c. i. of oxigen gas, and form 200 c. i. of carbonic acid gas. By a very simple calculation we find, that 100 parts of carbon combine with 16·7597 of hidrogen at a *minimum*, and precisely twice as much at a *maximum*. We see by the analysis of sulphuretted hidrogen gas, already mentioned, that 100 parts of sulphur combine with 6·66 of hidrogen. If from these data we endeavour to calculate the degree of oxidation of sulphur that answers to the gaseous oxide of carbon in the following manner, 16·7597 : 125·818 : 6·66 : 49·997, we perceive, that there is a point of oxidation of sulphur, in which 100 parts of sulphur are combined with 50 of oxigen very nearly.

Composition of inflammable gasses.

Sulphuretted muriatic acid.

On examining Mr. A. Berthollet's experiments on sulphuretted muriatic acid, if I may be allowed the term*, we see, that 100 parts of sulphur had condensed 204 of oximuriatic acid, containing 47·67 of oxigen. In the experiments of Messrs. Bucholz and Gehlen care was taken to combine with the acid the greatest quantity of sulphur possible; and 100 parts of sulphur yielded 211 of the mixture: so that 100 parts of sulphur were combined with 25·19 of oxigen and 85·91 of muriatic acid. Admitting, that Mr. Berthollet must have had 214 parts of oximuriatic acid combined with 100 of sulphur, and Messrs. Bucholz and Gehlen 107 parts combined with the said quantity, we have two oxides of sulphur, one of which is composed of 100 sulphur and 25 oxigen, the other of 100 sulphur and 50 oxigen. Thus the compounds of sulphur with muriatic acid form a muriate of the oxidule of sulphur, and a muriate of the oxide. From

Two oxides of sulphur.

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* Certainly Dr. Berzelius need not scruple to use the name given to this compound by its discoverer, Dr. Thomson, for whose account of it see Journal, vol. VI, p. 104. C.

this view of things I have concluded, that the degrees of oxidation, which appear to be multiplications by 1½, are in fact only multiplications by 6 or 12 of a degree of oxidation at a *minimum*, which is not known, because it cannot exist in a separate ſtate.

Supposition, that ammonia contains no oxigen.

I have lately read in the Philosophical Annals of Messrs. Gilbert a paper by Messrs. Thenard and Gay-Lussac, which appears to prove, that the amalgam of ammonia is a compound of mercury, alkali, and hidrogen. I cannot however be of their opinion; for, having demonstrated by incontestible experiments the oxidation of the metalloids of potash and soda, it would be highly inconsistent to suppose, that ammonia alone ſhould exhibit phenomena so similar in outward appearance to those of the fixed alkalis, earths, and metallic oxides, while intrinsically they were of a totally different nature. I am convinced, therefore, that the substance in ammonia, which forms an amalgam with mercury in the circuit of the pile, is a metal as indecomposable as the others. But, supposing this, it naturally follows, that hidrogen and nitrogen muſt be its oxides, as Mr. Davy had already supposed*. From the laws that I have endeavoured to establish it would be easy to determine the quantity of oxigen, that enters into each. If, as I have endeavoured to prove, ammonia is composed of 100 base to 89·4 of oxigen, we shall find the quantity of oxigen, which with 100 parts of the base forms hidrogen, by dividing 89·4 by 2, 4, or 8. The quantity of oxigen necessary to convert these 100 parts of the base into nitrogen will be 89·4 multiplied by 1·5, 2, 4, &c.

Hidrogen and nitrogen considered as two different oxides of the metallic base of ammonia

Compounds of ammonium and oxigen.

We shall have found the true proportions, when the quantity of hidrogen and nitrogen gasses produced from ammonia by means of electrical discharges contain, according to these calculations, the same quantity of oxigen as ammonia. On dividing 89·4 by 8 we shall have the oxigen necessary to form hidrogen with 100 parts of the base; and on multiplying 89·4 by 1·5 we shall have the quantity required for the formation of nitrogen. On reducing the measures of gas to weights, we shall find, that 16·66 grs. of ammonia yield 14·85 of nitrogen, and 3·81 of hidrogen, in which we shall find, according to the calculation above-mentioned, 8·8 of oxigen. It is very natural, that I should hitherto have been able to obtain only tolerably near approximations to the truth, and thus trifling differences may exist, without the principle, on which I have founded this calculation, being erroneous. According to this 100 parts of the base of ammonia, which I shall call ammonium, combine with 11·175 of oxigen, to form hidrogen. This quantity I shall express by 1 *ox.* The combination of 44·7 = 4 *ox.* (oxidule of ammonium) exists in all probability in the olive-coloured substance formed by the contact of potassium with ammoniacal gas. That of 89·4 = 8 *ox.* forms ammonia: and 134·1 = 12 *ox.* forms nitrogen, which ought therefore to be composed of 57·28 oxigen and 42·72 ammonium. From the corresponding analysis of Messrs. Davy and Gay-Lussac, 100 parts of nitrogen combine with 57·3 of oxigen to form the oxidule of nitrogen [nitrous oxide]: but these hundred parts of nitrogen contain 57·3 of oxigen, so that in the oxidule of nitrogen ammonium is combined with twice as much oxigen as in nitrogen; that is, 100 parts of ammonium are combined with 268·2 = 24 *ox.* According to the analyses already quoted 36 *ox.* produce nitrous gas, and 60 *ox.* nitric acid. Between 36 and 60 the proportion of 48 is wanting, which, according to all appearance, belongs to nitrous acid. 76 *ox.* produce water, but this number stands too much alone to be verified by any calculation*.

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* Dr. Davy has since been inclined to relinquish this supposition. C.

Hidrogen evolved from

Another circumstance, that has appeared difficult to explain, is, that potassium evolves nearly the same quantity of hidrogen in ammonia as in water. If the experiments of Mr. Davy be as accurate as they appear, 100 parts of potassium

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Proportion of oxigen to hidrogen in water.

* The proportion of oxigen to hidrogen in water, on which Mr. Berzelius appears to have formed his calculation, is 88·25 to 11·75: but, if we take the oxigen in water = 72 *ox*, to use his expression, which is just double 36 *ox.*, the proportion of oxigen to hidrogen in 100 parts of water will be 87·7 to 12·3 very nearly. This differs but a trifle from the conclusions of von Humboldt and Gay-Lussac, adopted by Mr. Dalton in his Chemical Philosophy, Part II, p. 274, 5: and the mean between the two will be a near approximation to 87·5 and 12·5, which are precisely in the ratio of 7 to 1. C.

tassium decompose 37·8 of ammonia. The potassium then forms an oxidule, combining with 10·5 of oxigen, and it also reduces the ammonia to the state of oxidule. But these two oxidules must be combined in such proportion, that one contains twice or thrice as much oxigen as the other. If we admit, that the oxidule of potash contains thrice as much oxigen as that of ammonia, it follows from this calculation, which cannot be perfectly accurate, that the potassium must produce a quantity of hidrogen exceeding in a very trifling degree what it evolves from water.

ammonia by potassium.

Table of the Analyses in the little Treatise I have the Honour to transmit to you.

Analysis of various salts.

Oxides of lead.	
—— Yellow	Lead, 100 parts: oxigen, 7·7
—— Red	—————— 11·1
—— Brown	—————— 15·4
Sulphuret of lead	———— Sulphur, 15·445
Sulphate of lead	Sulphuric acid, 100: oxide of lead, 280
Muriate of lead	Acid, 100; oxide of lead, 421·4
Carbonate of lead	Acid and water, 16·5: oxide of lead, 83·5
Sulphurous acid	Sulphur, 100: oxigen, 99·8
Sulphuric acid	———— ——149·6
Sulphate of barytes	Acid, 100: base, 194
Sulphite of barytes	—— 86·53: water 4·25: base, 209·22
Carbonate of barytes	—— 21·6: base, 78·4.
Sulphuret of copper	Copper, 100: sulphur, 25·6
Oxidule of copper	Copper, 100: oxigen, 12·5
Oxide of copper	———— —— 25
Sulphate of copper	Acid, 49·1: oxide, 50·9
Sulphate of oxidule of copper	—— 100: oxidule, 183
Muriate of oxidule of copper	—— 100: —— 278·4
Neutral muriate of oxide of copper	—— 100· oxide, 148·7

Submuriate

Analysis of various salts

Submuriate of oxide of copper ··········	acid 100: oxide 596
Muriate of barytes ····	—— 100: barytes, 288·2
——— silver ····	—— 18·7 : oxide, 81·3 —— 100: —— 434·8
Oxide of silver ······	Silver, 100 : oxigen 7·9
Sulphuret of iron at a minimum ········	Iron, 100: sulphur 58·75
——— at a maximum ········	—— 100: —— 117
Sulphate of oxidule of iron ··············	Acid, 100 : oxidule, 88
Neutral sulphate of oxide of iron ········	—— 100 : oxide, 65·5.
Subsulphate of oxide of iron ·············	—— 100 : —— 266
Oxide of iron ········	Iron, 100 : oxigen, 44·25
Oxidule of iron ······	—— 100 : —— 29·5
Potash ··············	Potassium, 100 : oxigen 20·49
Sulphate of potash ····	Acid, 100 : potash, 112·35
Muriate of potash ····	—— 100: —— 179
Soda ··············	Sodium, 100 : oxigen, 34·6
Sulphate of soda ····	Acid, 100 : soda, 79·34
Muriate of soda ······	—— 100 : ——118·627
Ammonia ··········	Ammonium, 100 : oxigen, 189·4
Muriate of ammonia ··	Acid, 49·46 : water, 18·59 : base, 31·95 : or, acid, 100 : base, 64·6
Lime ··············	Calcium, 100 : oxigen, 39·86
Sulphate of lime ·····	Acid, 100 ; lime, 72·41
Muriate of lime ······	—— 100 : —— 107·9
Barytes ············	Barium, 89·5 : oxigen, 10·5
Oximuriatic acid ····	Acid, 100 : oxigen, 30·49
Common muriatic acid gas ··············	—— 100 : water, 34·5
Oxide of zinc ········	Zinc, 100 : oxigen, 24·4
Water ··············	Hidrogen, 100 : oxigen, 750·77 : or ——— 11·754 : —— 88·246
Sulphuretted hidrogen gas ·············	——— 100 : sulphur, 1501·54 : or —— 6·247 : —— 93.753

V.

Account of a Substitute for Leghorn Plait, for Hats, &c.
By Mr. WILLIAM CORSTON, *of Ludgate Hill**.

DEAR SIR,

Manufacture of Leghorn plait flourishing.

HAVING been honoured, in May 1805, with the gold medal of the Society, for a substitute of Leghorn plait for hats, it is with great satisfaction that I am able to inform you, that this country is now beginning to reap those advantages, which I foretold to the Society six years ago, and that many hundreds of women and children are at present employed in the various parts of this kingdom, in the manufacture of this article.

I sold to two persons, in less than two months, upwards of 5000 scores, and had an order from a third for 2000. But this bears but a small proportion to the demand, and evinces the truth of the statement I made of the great advantages likely to result from the introduction of this new branch of manufacture into this country.

Application of waste land,

In Joseph Lancaster's Book on Education, I have pointed out farther advantages, which may be derived by the country at large, from the cultivation of waste and barren lands for the production of the material of which the British leghorn is made. This has been proved by experiments, which I have made on Bagshot Heath, by favour of the Earl and Countess of Harcourt; and in Bedfordshire, by the benevolence and public spirit of the Duke of Bedford; and on barren land in Norfolk, near my native place. Indeed no soil can be too barren for this purpose, provided the seed will lie. I have shown, that 2000 acres might be annually cultivated in the growth of this article, and that a quantity of such land might in succeeding years be brought into more productive cultivation: but I am afraid, that this plan is and employment for poor children.

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* Trans. of the Soc. of Arts, &c. vol. XXVIII, p. 130.

too simple te be adopted; although I cannot but yet hope, that the agricultural societies of England will turn their attention to a plan, which will bring waste lands into cultivation, and also provide employment for thousands of poor children. If government would grant 3000 acres of the land, which lies waste on Bagshot Heath, for a few years, without any fine, and afterward on an increasing rent, according to the improvements of the soil, I would raise, in straw alone, what should produce an article for industry, for which upwards of £20000 would be paid annually for the employment of poor children. It is a pleasing sight for Englishmen to behold the superb buildings which are appropriated as asylums for the children of our soldiers and sailors; but in times like these, how desirable is it, that buildings of only one story high ſhould be erected in populous parishes, which might answer the double purpose of schools of industry and instruction, and thereby relieve parishes from the burden of the maintenance of poor children, and also bring them up in habits of industry and sobriety! In this way thousands of children may be employed from seven years of age, until they arrive at an age sufficiently advanced to go out as servants.

raw manu-
ture.

As by the mere invention of the *splitting of a straw*, a source of employment has been discovered, which has increased the returns in that branch not less than 3 or £400000 annually, I feel myself urged to call the attention of the discerning part of the public to a new branch of industry, which I make no doubt will, in a very few years, add nearly an equal sum to the national industry, and also be a great means of bringing into cultivation thousands of acres of land now lying waste. Since the introduction of spinning by hand, no source of employment has been discovered, which promises to afford occupation to so many thousands; spinning by hand has been superseded by the inventions of machinery, but I believe it to be impossible for machinery to absorb this branch of manual industry;
lles, wheels, or bobbins, engaged in this work,
t, the fingers of little children.
us may endeavour to cast a shade over these
by considering the prevalent attachment to
the

the wear of straw hats as the whim of the day; but, I believe, that the superior comfort, in summer weather, arising from the wear of a light hat in preference to a heavy one, will induce gentlemen more and more to make use of the British leghorn; and as to the predilection of ladies for hats manufactured of split straw, I think I hazard very little in considering that as established; and when to our home consumption is added a consideration of the demand for the East and West Indies, the coast of the Mediterranean and South America, I think myself very safe in asserting, that these manufactures will employ not less than 60000 children.

Poor rates.

Our poor's rates amount to more than five millions per annum; and there can be no remedy for so great a burden equal to the setting the children of the poor to work, so that they shall earn their own bread, instead of being chargeable to the parish. It is true, that the demand for straw-plait has caused an increased quantity to be made; yet the demand is still superior to the quantity; and in the spring, the price often advances from 30 to 50 per cent beyond its fair value, even allowing sufficient profit to the poor employed, and the dealer in the article. I believe, therefore, that this branch of manufacture is still in its infancy, and that it is likely to have great permanency; and although it may, by some, be considered as an insignificant source of revenue, yet when it is considered, that Providence has given us the means of improving the agricultural state of the kingdom, in raising the raw materials, and that so many thousands of our poor may be employed in its manufacture, I trust that every assistance will be afforded to so extraordinary a source of national wealth.

The manufacture of straw still in its infancy.

The value almost wholly from the labour.

If any person should doubt my arguments, I will beg leave to state a fact in confirmation of my positions. I once had the curiosity to put into the scale some straw I was about to sell, and I found that it netted upwards of twenty three pounds sterling per lb. weight. If therefore, an article, which in its unmanufactured state is considered as of little worth, can, merely by the industry of children, be rendered so valuable, I think I risk very little in affirming, that by the encouragement of the *British Leghern*, together with that of *split straw*, we gain a sure means of bringing our waste and barren lands into cultivation; and, by the employment of our poor children, we acquire an infallible means of greatly diminishing our poor's rates.

The rye should be sown on the most barren land.

In order that the British plait may equal the Italian in fineness, I particularly recommend, that the rye should be sown on the most waste and barren land, without any reference to its produce but merely of the straw, the sale of which would afford ample remuneration; and I should be happy to take the produce of from 50 to 100 acres of such land, provided it lay convenient to the place of my manufactory. By such means, the most unproductive wastes will become valuable, and a great source of advantage opened for the employment of young children, and persons incapable of hard work.

An opportunity is thus offered for benevolent persons to build cheap schools in villages, and assemble the children of the poor together, to whom literary instruction might be given, and the children enabled to earn their own bread; and the whole effected at a trifling expense.

I flatter myself, that it will give pleasure to the Society to find, that I have not neglected an object, which has merited their attention; and which will be the means of saving immense sums to this country, which have heretofore been sent abroad for the purchase of an article, which our poorest lands and feeblest people can furnish. I remain, dear Sir,

Your obliged and obedient Servant,

WILLIAM CORSTON.

Ludgate Street, May 10, 1810.

VI.

Correspondence of Dr. WILLIAM ROXBURGH, *of Calcutta, with Dr.* C. TAYLOR, *Secretary to the Society of Arts, &c. on various Drugs.*[*]

SIR,

East India fever bark, swietenia febrifuga.

I HAVE the pleasure to send you a quantity of my East India fever bark, discovered by me about fifteen years ago; since which period it has had numerous fair trials in many parts, which have been attended with every success that could be wished as a substitute for Peruvian bark, for which I first ventured to propose it.

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[*] Trans. of the Soc. of Arts, vol. XXVIII, p. 308.

A figure and description of the tree, which furnishes this bark, have been published under the name of *swietenia febrifuga*, in my account of Coromandel plants, vol. 1, page 18, table 17. It is a large timber tree, a native of the various mountainous parts of India. You will observe, that this bark possesses an agreeable odour, and from numerous experiments, which I have made with fresh bark, I have drawn the following conclusions:—

Its properties.

1. That the active parts of the bark of swietenia febrifuga are much more soluble than those of Peruvian bark, particularly in watery menstruums.

2. That it contains a much larger proportion of active, bitter, and astringent powers, than Peruvian bark.

3. That the watery preparations of this bark remain good much longer than similar preparations of Peruvian bark.

4. That the spirituous and watery preparations bear to be mixed in any proportion, without decomposition.

5. That this bark, in powder, and its preparations, are more antiseptic than Peruvian bark, or similar preparations thereof.

Method of administering it.

In my practice I generally gave from twenty to sixty grains of the fine powder in substance, either in wine or water, as circumstances required, and commonly as often as Peruvian bark is usually prescribed.

It may be imported at a low rate.

I recommend, that some of this bark may be sent to the fenny countries, where intermitting fevers prevail; and if it is found to answer, which I have no doubt of, it may be imported from the East Indies at so low a rate, as to render its use very general, on account of the high price of Peruvian bark.

I am, Dear Sir,
Your most obedient Servant,
March 28, 1806. W. ROXBURGH.

Experiments in England.

⁂ From experiments since made in England, the swietenia bark has been found a valuable medicine in intermittent fevers, scrofula, and in disorders usually termed nervous.

DEAR SIR,

I wrote to you lately, along with my papers on the manufacture of indigo, and of some newly discovered plants, which yield that drug.

A cheap resin from the saul, or shorea robusta.

It appears to me now, that it will tend to a useful purpose to put the Society in possession of samples of a very cheap resin, the produce of one of our largest and best timber trees, called by the natives of Bengal, *saul*, and by me, *shorea robusta.* It is one of the substances used in our Indian naval yards under the general name *dammer*, and is

Its uses.

a substitute for pitch and tar. To bring it to a proper consistence for such use, it is boiled up with some cheap vegetable oil, (the Hindoos being forbidden by their religion to use any animal oil), and more or less of the vegetable oil is added, according to the purpose for which it is wanted. The Society will probably find it also applicable to other uses, as it is a pure resin, cheap and plentiful: the price of it here is from three halfpence to two pence per pound. I wish to know, whether it has been yet known in England, and whether it is likely to be in demand. It will probably be useful for making sealing-wax, and for varnish.

I am, my dear Sir,
Yours very obediently,

Calcutta, Jan. 18, 1809. W. ROXBURGH.

MY DEAR SIR,

Black myrobalans.

I have now sent to you farther samples of the resin of my shorea robusta; and I have also sent a parcel of the black myrobalans, *(myrobalanus Indica),* the origin of which has hitherto been unknown. I believe, that they are the unripe fruit of the same tree, which produces the *chebulic myrobalans*; and you will trace the cause of my now having discovered the tree which produces them in part 3 of the eleventh volume of the Asiatic Researches, containing a catalogue of Indian medicinal plants and drugs, with their names in the Hindustani and Sanscrit languages, by John Fleming, M.D., pages 29, 30, and 31, which the author sends to you for the Society. But though their medicinal virtues are in high repute over Asia, I do not send them to you with that view

alone,

They contain much tannin.

alone, but rather because I think they contain much *tannin* in little bulk, and may therefore be useful, and save the British oak plantations. I fear the gaub extract, from the fruit of *embryopteris glutinifera*, which I sent you some time since for the trial of the tanners, may not have answered so well as I expected, otherwise that you would have applied for more of it.

Hurra, the fruit of terminalia chebula.

I take the present opportunity to request you will correct a mistake in my letter of June 18, 1804, published in the 23d volume of the Society's Transactions, page 403, where I said *hurra* was the fruit of *terminalia citrina*; I now find it is the fruit of *terminalia chebula*.—See Coromandel plants, 2, No. 197, and of Wildenouw's edition of the Species Plantarum, 4, 969. I now send to you a drawing and description of the tree, and of the myrobalans in their various stages, both fresh from the tree and dried as mentioned by Dr. Fleming. The small parcel within the other contains some of the drug purchased in the bazar, viz. four pounds weight, for which I paid one shilling. The remainder are fresh gathered from two trees in this garden, and hastily dried in the sun; they are rather advanced, and may answer to the fourth, fifth, and sixth sorts of the drawing; and among those of the bazar will be found the three first. I have also sent you some more fever bark, part of the produce of a young tree which grew in this garden. It is difficult to judge how long we may be conveniently supplied with Peruvian bark; and it is therefore very proper, that this valuable substitute should be brought into general use as soon as possible, and if it is likely to meet with extensive demand, I will contrive that some of it be sent home for sale.

New species of brucea.

In the same package is enclosed some bark of a new species of *brucea*, which is said to be a most powerful medicine; it is the *lussa radga* of Rumphius's Herbarium Amboinensis, 7, p. 27, t. 15: it is a thin bark, and may probably be as good or better than *simarouba*. In the same bundle is another parcel, which is the *conessi bark* of our *Materia Medica*; it has an austere bitter taste, and is recommended in dysenteries, diarrhœas, &c., as an astrin-

Conessi bark.

gent*. I wish to receive the opinion of the Society on these and other articles, which I have sent.

I am, dear Sir,
Yours very obediently,
Calcutta, Oct. 3, 1809. W. ROXBURGH.

MY DEAR SIR,

Gaub extract.

Captain Richardson having been detained thus long, and this being the season for the gaub fruit, I have made a few pounds of the extract, which is packed in the same box with the articles mentioned in my former letter. At the bottom of the box there are ten pounds made with cold water. Immediately above it is another stratum, weighing six pounds and a half, made with hot water from the refuse left after the cold water process. These two parcels, with that I sent you formerly, will certainly enable the Society to ascertain and let me know what prospect of success this extract holds out to your tanners. I request the Society will order experiments to be made therewith as early as possible, and I anxiously wait for letters from you acquainting me with the result.

I remain, dear Sir,
Yours truly,
Calcutta, Nov. 21, 1809. W. ROXBURGH.

Samples for trial.

*** Samples of the several articles above mentioned will be delivered for trial to such persons, as will engage to favour the Society with the result of their experiments thereon.

VII.

Demonstration of the Fundamental Property of the Lever, by DAVID BREWSTER, *LL.D. F.R.S. Edin.*‡

The fundamental pro-

IT is a singular fact in the history of science, that, after all the attempts of the most eminent modern mathematicians, to obtain a simple and satisfactory demonstration of the fundamental property of the lever, the solution of this problem given by Archimedes should still be considered as the most legitimate and elementary. Galileo, Huygens, De la Hire, Sir Isaac Newton, Maclaurin, Landen, and Hamilton, have directed their attention to this important part of mechanics; but their demonstrations are in general either tedious or abstruse, or founded on assumptions too arbitrary to be recognised as a proper basis for mathematical reasoning. Even the demonstration given by Archimedes is not free from objections, and is applicable only to the lever considered as a physical body. Galileo, though his demonstration is superior in point of simplicity to that of Archimedes, resorts to the inelegant contrivance of suspending a solid prism from a mathematical lever, and of dividing the prism into two unequal parts, which act as the power and the weight. The demonstration given by Huygens assumes as an axiom, that a given weight, removed from the fulcrum, has a greater tendency to turn the lever round its centre of motion; and is, besides, applicable only to a commensurable proportion of the arms. The foundation of Sir Isaac Newton's demonstration is still more inadmissible. He assumes, that, if a given power act in any direction upon a lever, and if lines be drawn from the fulcrum to the line of direction, the mechanical effort of the power will be the same when it is applied to the extremity of any of these lines; but it is obvious, that this axiom is as difficult to be proved, as the property of the lever itself. Mr. De la Hire has given a demonstration which is remarkable for its want of elegance. He employs the *reductio ad absurdum*, and thus deduces the proposition from the case where the arms are commensurable. The demonstration given by Maclaurin has been highly praised; but if it does not involve a *petitio principii*, it has at least the radical defect of extending only to a commensurable proportion of the arms. The solutions of Landen and Hamilton are peculiarly long and complicated, and resemble more the demonstration of some of the abstrusest points of mechanics, than of one of its simplest and most elementary truths.

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* See farther particulars of this bark in Dr. Roxburgh's Treatise of Nerium Indigo, p. 254 of this volume, under the name of *Nerium Antidysentericum.*

‡ Trans. of the Roy. Soc of Edinb. vol. VI, p. 373.

perty of the lever never satisfactorily demonstrated.

Attempted by

Archimedes,

Galileo,

Huygens,

Sir I. Newton,

De la Hire,

Maclaurin,

Landen, and Hamilton.

In attempting to give a new demonstration of the funda-

A new demonstration attempted.

mental property of the lever, which shall be at the same time simple and legitimate, we shall assume only one principle, which has been universally admitted as axiomatic, namely, *that equal and opposite forces, acting at the extremities of the equal arms of a lever, and at equal angles to these arms, will be in equilibrio.* With the aid of this axiom, the fundamental property of the lever may be established by the three following propositions.

Axiom.

Propositions

In Prop. I, the property is deduced in a very simple manner, when the arms of the lever are commensurable.

In Prop. II, which is totally independent of the first, the demonstration is general, and extends to any proportion between the arms.

In Prop. III, the property is established, when the forces act in an oblique direction, and when the lever is either rectilineal, angular, or curvilineal. In the demonstrations which have generally been given of this last proposition, the oblique force has been resolved into two, one of which is directed to the fulcrum, while the other is perpendicular to that direction. It is then assumed, *that the force directed to the fulcrum has no tendency to disturb the equilibrium, even though it acts at the extremity of a bent arm*; and hence it is easy to demonstrate, that the remaining force is proportional to the perpendicular drawn from the fulcrum to the line of direction in which the original force was applied. As the principle thus assumed, however, is totally inadmissible as an intuitive truth, we have attempted to demonstrate the proposition without its assistance.

General assumption

here dispensed with.

Prop. I.

Prop. I. *If one arm of a straight lever is any multiple of the other, a force acting at the extremity of the one will be in equilibrio with a force acting at the extremity of the other, when these forces are reciprocally proportional to the length of the arms to which they are applied.*

Demonstrated.

Let AB (Plate VIII, fig. 1,) be a lever supported on the two fulcra F, *f*, so that A *f*=*f*F=FB. Then, if two equal weights C, D, of 1 pound each, be suspended from the extremities A, B, they will be in equilibrio, since they act at the end of equal arms A*f*, BF; and each of the fulcra *f*, F, will support an equal part of the whole weight, or 1 pound.

Let the fulcrum f be now removed, and let a weight E, of 1 pound, act upwards at the point f: the equilibrium will still continue; but the weight E, of 1 pound, acting upwards at f, is equivalent to a weight G of 1 pound, acting downwards at B. Remove, therefore, the weight E, and suspend the weight G from B; then, since the equilibrium is still preserved after these two substitutions, we have a weight C, of one pound, acting at the extremity of the arm AF, in equilibrio with the weights D and G, which together make two pounds, acting at the extremity of the arm FB. But FA is to FB as 2 is to one; therefore an equilibrium takes place, when the weights are reciprocally proportional to the arms, in the particular case when the arms are as 2 to 1. By making Ff successively double, triple, &c. of FB, it may in like manner be shown, that, in these cases, the proposition holds true.

Prop. II.

Prop. II. *If two forces, acting at the extremities of the two arms of a lever, and at equal angles to the arms, are in equilibrio, they will be reciprocally proportional to the lengths of the arms to which they are applied.*

Demonstrated.

Let AB, CD (fig. 2,) be two levers in contact at AB, and forming one straight line, ABCD. Bisect AB in f, and CD in ϕ, and from the extremities A, B, suspend equal weights m, m, and from the extremities C, D, equal weights n, n, so that $m:n=CD:AB$. If the two levers are now supported on the fulcra f, φ, they will both be in equilibrio, and will still form one straight line, the fulcrum f being loaded with a weight $=2m$, and the fulcrum ϕ with a weight $=2n$. Let us now suppose the extremities B, C, of the levers to adhere, and form one inflexible line AD; and let an inverted fulcrum F be placed at the point of junction. The equilibrium of the whole will evidently continue, and the fulcra f, φ, will be loaded as before. Remove the fulcra f, φ, and substitute in their place the weights $2m$, $2n$, acting upwards, and equal to the load which they respectively support: The equilibrium will still continue. Now, instead of the force m acting downwards at B, substitute an equal and opposite force m', acting upwards at A, and instead of the force n acting downwards at C, substitute an equal and opposite force n, acting upwards at D, and the equilibrium will still be preserved. But the two equal forces acting in opposite directions at the points A and D destroy each other; therefore we have a force $2m$ acting at the extremity of the arm fF, in equilibrio with a force $2n$, acting at the extremity of the arm ϕF. But since, by the hypothesis, $m:n$ as $CD:AB$, and since fF is one half of AB, and ϕF one half of CD, we have $2m:2n=\phi F:fF$, an analogy which expresses the fundamental property of the lever.

Lemma.

Lemma. *Two equal forces acting at the same point of the arm of the lever, and in directions which form equal angles with a perpendicular drawn through that point of the arm, will have equal tendencies to turn the lever round its centre of motion.*

Demonstrated.

Let AB (fig. 3,) be a lever with equal arms AF, FB. Through the points A, B, draw AD, BE, perpendicular to AB, and AP, Ap, BW, Bw, forming equal angles with the lines AD, BE. Produce PA to M. Then, equal forces acting in the directions AP, Bw, will be in equilibrio. But a force M equal to P, and acting in the direction AM, will counteract the force P, acting in the direction AB, or will have the same tendency to turn the lever round F; and the force W, acting in the direction BW, will have the same tendency to turn the lever round F as the force M; consequently the force W will have the same tendency to turn the lever round F as the force w.

Prop. III.

Prop. III. *If a force acts in different directions at the same point in the arm of a lever, its tendency to turn the lever round its centre of motion will be proportional to the perpendiculars let fall from that centre on the lines of direction in which the force is applied.*

Demonstrated.

Let AB, (fig. 4,) be the lever, and let the two equal forces BM, Bm, act upon it at the point B, in the direction of the lines BM, Bm. Draw BN, Bn, respectively equal to BM, Bm, and forming the same angles with the line PBp perpendicular to AB. To BM, Bm, BN, Bn, produced, draw the perpendiculars AY, Ay, AX, Ax. Now, the side AX=AY, and Ax=Ay, on account of the equality of the triangles ABX, ABY; and if Ml, $M\lambda$, be drawn perpendicular to $B\omega$, the triangles ABY, BMl, will be similar. and also the triangles ABy, $Bm\lambda$: Hence we obtain

$$AB : AY = BM : Bl, \text{ and}$$
$$AB : Ay = BM : B\lambda$$

Therefore, *ex æquo*, $AY : Ay = Bl : B\lambda$

Complete the parallelograms $BMoN$, $Bm\omega n$; and Bl, $B\lambda$ will be respectively one half of the diagonals Bo, $B\omega$.

Now let two equal forces BM, BN, act in these directions upon the lever at B, their joint force will be represented by the diagonal Bo, and consequently one of the forces BM will be represented by $Bl = \frac{1}{2} Bo$. In the same manner, if the two equal forces Bm, Bn, act upon the lever at B, their joint force will be represented by $B\omega$, and one of them, Bm, will be represented by $B\lambda = \frac{1}{2} B\omega$. Consequently the power of the two forces BM, Bm, to turn the lever round its centre of motion, is represented by Bl, $B\lambda$, respectively; that is, the force BM is to the force Bm as Bl is to $B\lambda$; that is, as AY is to Ay, the perpendiculars let fall upon the lines of their direction.

VIII.

On the Nature of those Meteors commonly called Shooting Stars. In a Letter from JOHN FAREY, *Senior, Esq.*

To WILLIAM NICHOLSON, Esq.

SIR,

IN several Meteorological Reports of late, and particularly in a letter from Mr. Thomas Forster, at page 131 of your October number, the appearances usually denominated *shooting stars* are noticed, and treated of as being a phenomenon connected with the electric and other particular state of our atmosphere, particularly "clear *dry* weather "and easterly winds," "clear frosty winter nights," and "the clear intervals of showery weather." Now these three

Shooting stars supposed to be connected with electrical or other states of our atmosphere.

Its clearness apparently re-

states

cessary to our seeing them.

states of the air are best adapted, by its clearness, for seeing the smaller stars and planets, or any small distant object by land; and I wish particularly to call the attention of your Meteorological Correspondent to notice, whether the *absence* of the *twilight, moon-light*, &c. is not equally essential to seeing numbers of the small rapidly shooting-stars; as I certainly found them to be, in a series of observations continued for more than a year in 1800 and 1801, in conjunction with an able friend at 6 miles distance; and whence it seemed ascertained, that these phenomena are occasioned by an almost infinite number of *satellitulæ*, or very small moons, constantly revolving round the Earth, in all possible directions, and appearing only during the very short time that they dip into the upper part of the atmosphere each time that they are in *perigee*: and that no step seems wanting in the degree of this dip into the atmosphere, and their consequent brightness, length, and slowness of courses, &c. between the smallest instantaneous *shooting-stars*, and the largest *meteors*, (such as that of August, 1783, alluded to by your correspondent,) which throw off with explosions angular fragments of metallic and stony matters, that so frequently fall to the Earth, as *meteoric stones*. The long trains or streaks of light, often mentioned as *left* by meteors for some instants, will frequently be found mere optical deceptions, owing to the eye not following the meteor, but suffering it to cross the field of sight, where its impression is left, on known optical principles.

Probably they are satellitules, of which an infinite number move in all directions round the Earth,

from the smallest to the largest meteors.

Hoping to see this important class of phenomena more closely and extensively investigated than they hitherto have been,

I remain, Sir,
Your obedient humble Servant,
JOHN FAREY, Sen.

Upper Crown Street, Westminster,
5th Nov. 1811.

IX.

On the Causes of the Decay of the Timber in Ships, and the Means of preventing it. In a Letter from a Correspondent.

To WM. NICHOLSON, Esq.

SIR,

Shipping of great importance to this country.

THE advantages that England derives from her marine, whether considered as appertaining to commerce or defence, are too well known to need any comment; whatever then will contribute either to the safety or durability of the navy becomes a matter of great public importance.

Causes of the decay of the timber of.

The grand cause of the decay of the timber employed in building of ships is the decomposition of its substances by putrefaction, which is occasioned by moisture. This precautions and management may retard, but not prevent; but a secondary one, the dry rot, may, I think, be both prevented and eradicated.

The dry rot known in very ancient times.

Remedies for it.

The dry rot, as it is usually called, proceeds from the growth of a parasitical plant, named by botanists *boletus lachrymans*, which belongs to the class of cryptogamia. Its injurious tendency is mentioned as far back as history will carry us, and the appearance and ravages are particularly pointed out in the Bible*. The cure there directed is, to remove the materials injured; and, if this did not stop the disease, the house was razed, and the entire articles of which it was composed taken without the city. In latter times an equally effectual but more easy remedy has been applied in buildings, where this plant has taken root; that of causing a circulation of air in the parts affected; but this cannot be introduced in the fabrics of which we are now treating.

Its injuriousness to the navy formerly.

The fatal tendency of the dry rot in ships cannot be pointed out in a more forcible way, than it is in the memoirs of Pepys, who was secretary to the Admiralty during the reigns of Charles the 2nd, and James the 2nd. At that time a commission was formed to inquire into the state of the navy, by which it appeared, that there were thirty ships, called new ships, which, as he observes, "for want of proper care and attention, had toadstools growing in their "holds as big as his fists, and were in so complete a state of "decay, that some of the planks had dropped from their "sides." From that time to the present, the evil has in some measure existed; and, although it has not since appeared in so great an extent as it then did, yet the state of some ships recently launched both justifies and demands all possible inquiry as to the causes of the growth of this fungus, and its prevention. Several means have been tried to prevent its vegetating, many of which might have answered this purpose, had they not been found to introduce evils as great as that which they pretended to cure. Among the most prominent, was the mode practised on the timbers of many ships, between the years 1768 and 1773, by saturating them with common salt; but this was found to cause a rapid corrosion in the iron fastenings, and the ships were (between decks) in a continual state of damp vapour. Mundic, found in the mines in Devonshire, has been lately employed, in fusion, to eradicate the vegetation, and prevent its future growth; but time is required to prove its efficacy.

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* Leviticus, Chap. 14.

nd at present.

Attempts to remedy it

by salt

and mundic.

In the common mode of constructing ships there are several causes, which promote the growth of fungi. The accumulation and consequent fermentation of materials not sufficiently seasoned, divested too of a free circulation of air, and permitting sap to remain on the edges of the frames, generate carbonic acid gas to the prejudice of the timber, and which promotes the growth of this boletus. Mr. Humboldt has found by experiments, that eight or ten hundredths of carbonic acid gas, added to the air of the atmosphere, rendered it extremely fit for vegetation; and that the air in mines, and other subterraneous passages, was found in this state, which is very favourable to the germination of all plants of the class cryptogamia. The gas found in the openings between the timbers of ships affected with the dry rot has been proved, to be precisely what Mr. Humboldt has mentioned.

Production of carbonic acid gas injurious.

The means, that I propose to prevent or cure this evil, are twofold: charring the whole surfaces of the timbers, and the

Means proposed to prevent or cure the dry rot.

Charring the surface of the timber.

not an original idea.

the inner surfaces of the planks, of which the ships are composed; and causing some slight deviations to be made in the modes practised in building them. I do not pretend to originality, when I recommend charring of timber, either to add to its durability, or prevent the growth of parasitical plants; for the experience of ages has proved the incorruptibility of charcoal, whether buried in the earth, or exposed to the action of air or water. The beams of the theatre of Herculaneum, which were reduced to this state by lava, were found, after a period of nearly eighteen centuries, to be perfect. The piles, supposed to have been driven into the earth by order of Julius Cæsar, when he forded the Thames at Cowey Stakes, near Shepperton, were charred, and, when recently taken up, found in a complete state, free from decay! Among many other instances, that may be adduced, the practice, almost universally* adopted, of burning the ends of posts to be put into the ground, to prevent premature dissolution, may be added as an additional proof of the efficacy of this recommendation; and makes us lament, that it has not been generally introduced in fabrics, where so much timber, labour, and money, have been expended; and the hopes and expectations of government or individuals frequently disappointed, by their rapid decay.

Proofs of its efficacy in rendering timber durable.

Other advantages from it.

There are several other advantages, that will be obtained by burning the surfaces of timber. Rats, which are so destructive to ships, will not touch charcoal; nor will the white ants and cockroaches, so common in the Indies, commit their depredations on substances so prepared. If farther evidence of its utility, when employed only on a small scale, be necessary, the durability of the Royal William, the flag ship at Spithead, which was built in the year 1719, and the planks *only* were burned on their inner surfaces, would be sufficient to prove its efficacy when practised on

Instances of the utility of its partial application in ships.

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* I am inclined to think, that the writer is mistaken here; and that the practice is very far from being even almost *generally* adopted. I remember a year or two ago speaking of it to a carpenter, who was putting down some posts; and he observed, that it would make them last too long, an object they never had in view in parish work. He added, that they sometimes charred the ends of posts, or more frequently dipped them in tar, for a private customer, "if he particularly desired it". C.

[illegible]

ships. Of late years the ends of ships' beams have been charred, and the sound state in which they are now found has justified and established the practice. Indeed all substances, that have undergone the action of fire, have been proved to be unfavourable to the growth of the boletus lachrymans; for, while stone has been rapidly destroyed by it, well burnt bricks, in the same buildings, and in nearly the same situation, have been free from its attacks.

From the scarcity of oak substitutes have been employed, chiefly pine.

Pitch pine generally durable,

but the soonest destroyed by fungi.

The scarcity of English oak, occasioned partly by the improved state of agriculture, but more by the increased numbers of our fleet, has obliged this country to have recourse to wood grown in other states. The principal that have been introduced in aid of oak are the varieties of American pine: it becomes therefore of some importance to inquire, which sort of this timber is the most durable, and which the soonest destroyed by vegetation. Pitch pine has been used by all nations in the construction of ships, and appears to be very superior to every other species for general durability; but this wood is the soonest destroyed by fungi, as these plants are nourished by the great quantity of resin contained in its numerous cells. I have lately seen some pitch pine plank of 7 inches in thickness completely decomposed; and, when cut open, the boletus was found to be vegetating in every part of it, but principally in the cells which were originally filled with resin. This proves how improper it will be to employ it as treenail fastenings, on which the strength and safety of ships so much depend.

Very improper for treenails.

Pitch pine should not be painted.

Preferable applications.

Pitch pine should not be covered with paint, as the pores of the wood are thereby stopped, and the expansion of the resin prevented, by which means the ligneous cells are broken, and decomposition takes place. The Americans pay the topsides of their ships with a mixture of oil, resin, &c., which are not unlike the substances that are contained in the wood they cover, and produce a hard varnish, impervious to water. Perhaps the preparation recommended by doctor Parry*, to prevent the dry rot, given in the Transactions of the Bath and West of England Societies, might be

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* It is made as follows: take 12 ounces of resin, 8 of roll brimstone, 3 gallons of oil, and 4 ounces of bees wax: boil them together, and lay them on while hot. [See Journal, vol. XIX, p. 337.]

Whitewashing between decks recommended.

be introduced also for this purpose with success. White wash or lime water to be used between decks is much to be preferred to paint, both on account of its cheapness and cleansing qualities, and also as it is detrimental to vegetation.

Alterations proposed in the mode of building ships.

Instead of the frames of a ship being converted to their proper shape for some months before they are put up, and afterward standing on a slip a year to season, as is now the usual practice; I would recommend, that they should be converted, and remain, together with the planks, in that state (under cover where there shall be a free circulation of air) for two years, then charred, put up, and the planking immediately begun; commencing operations from within board, by which means chips and dirt will not accumulate between the timbers; care being taken, that the holes be not bored too near the seams in the outboard plank. Holes should be bored, but no treenails driven till within a short time of the ship's being launched; this will both convey air within board, and carry off the vegetable juices, if any remain in the interior of the timbers. The planks could be kept in their places by the usual butt bolts, and some copper nails, or small bolts ragged, being driven at intermediate spaces. This too would strengthen the ship, as metallic fastenings are always to be preferred; in the wales and bottom, to treenails.

Remarks on bending the bow planks.

An objection may be made to the bringing round thick planks in the bow of a ship by burning, rather than the usual practice of boiling them in a kiln, on account of breaking their fibres. Although I do not see, that any difficulty can exist in the former method, as it is the usual practice of the French; yet, if any should occur on trial, and boiling them be considered absolutely necessary, vegetation may be prevented by dissolving some green vitriol in the water, and afterward fixing it in the wood by a weak alkali*. The method approved of by many judicious shipwrights, and constantly practised by the Dutch, that of

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* A solution of alum might also be tried—[I should apprehend any saline impregnation of the planks would prove injurious to the copper sheathing and fastenings. C.]

A trial might be instituted on a small scale.

sawing the thick planks, that are to be much bent, into two parts, might also be employed for this purpose. If a doubt should exist of the efficacy of wood prepared in these several ways to prevent the dry rot, specimens might be placed in a ship almost destroyed thereby, such a one as I recollect to have seen at Woolwich about 13 years since, which was in so bad a state, that the decks sunk with a man's weight, and the orange and brown coloured fungi were hanging in the shape of inverted cones from deck to deck. A few months trial of wood put into a ship so infected would prove the efficacy of either mode of preparation.

Exsiccation of timber in an oven, as recommended by Fourcroy, is also likely to add considerably to its durability.

One farther precaution is necessary. After a ship is built, she should lie at least six months in ordinary, with her hatchways covered to prevent the admission of rain water; some planks should be removed in the ceiling, and above the waterways of the several decks; and fires constantly kept in stoves, placed in the hold, and on the decks; by which means the moisture, that the charcoal may have attracted, will be dissipated, and the durability of the fabric insured.

Having stated these general circumstances with a view to prevent evils, which yearly exist to a great extent in the navy; I trust it will be the means of calling forth the opinions and abilities of those, whose minds have been directed, or whose occupations may lead them, to a consideration of this important subject.

I am, &c. &c.

11th of November, 1811. NAUTICUS.

X.

On the Art of Coating Metals with Platina: by Mr. GUYTON-MORVEAU.[*]

Platina may be applied on other metals in two ways.

THE application of platina on other metals less valuable, to prevent their oxidation, may be considered under two points of view, or as two different arts. The first of these may be called *platining* [*platinure*], as we say gilding, silvering: the other plating, a term appropriated by custom to a less superficial application, requiring a different process.

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[*] Ann. de Chim. vol. LXXVII, p. 297.

Platining may be executed like gilding, either by the intervention of mercury, or by means of a solution of muriate of platina in ether. That of washing.

I. I long ago made known the possibility of forming an amalgam of platina, and described the processes for obtaining it*. Mr. Proust, in a letter addressed to Mr. Vauquelin, inserted in the Ann. de Chim. pluviose, an. 12 [February, 1804], has said, that "hot mercury poured on the spongy substance remaining after the calcination of ammoniacal muriate of platina, dissolves it perfectly; and the result is a fatty amalgam, that does not grow hard by keeping, and spreads well on copper, gold, or silver; so that it might facilitate the plating of the former." By means of amalgam. Amalgam made by Proust,

From the note following this passage it appears, that Messrs. Fourcroy and Vauquelin also accomplished this amalgamation by the same process; that they even effected it without heat, and that, after having remained fluid for some time, it became very solid; an effect that might be accelerated by the application of a gentle heat. Fourcroy and Vauquelin,

Lastly, Mr. Hatchett published in Nicholson's Journal for October, 1804, a Letter, in which Count Mussin Poushkin gave him the particulars of the processes of amalgamation, by means of which he rendered platina perfectly malleable. and Count Mussin Poushkin.

At present therefore we cannot question the union of platina with mercury by means of simple processes, not expensive, and producing a suitable consistency for a solid application of the fixed metal: but it does not appear, that the processes of this new art have hitherto been published [in France] with any details; I shall therefore make known those described by Mr. Trommsdorff in the 7th vol. of his Journal, from the communication of Mr. Strauss†.

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* Ann. de Chim. January 1798, vol. XXV, p, 14, and fol.

† See also Nicholson's Journal, vol IX. [As the account that follows in the text is the same with that given at p. 30 of the vol. of the Journal quoted by Mr. Guyton-Morveau, it is omitted here as unnecessary. C.]

II.

By means of ether.

II. Another kind of platining, which appears particularly adapted to similar works of polished steel or iron, to prevent their rusting, is that which results from the application of platina to their surface by means of ether.

It is well known, that, if a solution of gold in nitro-muriatic acid be covered with sulphuric ether, and the two liquids shaken together, the ether will take the gold from the acid, acquire a yellow colour, and become capable of producing a true gilding, when applied to the surface of another metal.

The celebrated Lewis said, that platina would not form this union. Mr. Stodart supposes, that, if he did not effect the decomposition of muriate of platina by means of ether, it was probably because his platina was impure; and he has published in Nicholson's Journal* the process that succeeded in his hands.

* * * * *

Of plating or casing with platina.

Coating with platina.

From what has been said it appears, that the art of platining is not more difficult than gilding, and that it will have nearly the same advantage of preserving from rust those metals that are most liable to it. But at the same time it cannot be denied, that so thin a covering is far from promising the same durability, as that which is termed plating; particularly with respect to vessels and instruments continually exposed to the action of fire, or to being frequently rubbed.

Tried with success on copper 16 years ago.

I do not know, that plating with platina has yet been attempted in the large way: but there is every appearance that it would succeed as well as plating with gold or silver, and by the same well known processes. As a proof of this I have a small vessel in the shape of the bowl of a spoon, which was given me fifteen years ago by Professor Chabaneau, on his return from Spain, where he first introduced the principles of modern chemistry in his lectures†.

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* Vol XI, p. 282. [What follows in the text is omitted for the same reason as in the preceding instance. C.]

† Elementos de Ciencias naturales, &c. Madrid, 1790.

This vessel, 75 mil. [2·951 inch.] long, by 52 [2·046 in.] broad, and 14 [0·551 in.] deep, is made of copper, plated in the inside with platina. The thickness of its edges is 0·78 mil. [0·3 of a line]; it weighs 345·05 dec. [532·95 grs.]; and its specific gravity is 11·44.

As the metals here are only in juxtaposition, which can neither increase nor diminish their density, their respective proportions may be determined with precision from their specific gravity; and if we estimate that of platina at 21, and that of copper at 8·87, we shall find by calculation, that the vessel is composed of

Copper	0·766
Platina	0.234.

Thus the plating metal is a little more than a fifth, or in the most usual proportion of silver plating, the durability of which is established by use; though the properties of this metal in resisting the actions of heat and saline substances are very inferior to those of platina.

XI.

Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons; *By* B. C. Brodie, *Esq. F. R. S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.*[*]

1. THE following experiments were instituted with a view to ascertain, in what manner certain substances act on the animal system, so as to occasion death, independently of mechanical injury. I was led to the inquiry, from the subject of it appearing to be of considerable interest and importance, and from a hope, that, in the present improved state of physiological knowledge, we might be enabled to arrive at some more satisfactory conclusions, than had been deduced from any former observations.

Object of the following experiments.

The substances, which act as poisons when applied to the animal body, are very numerous. In the experiments, which I have hitherto made, I have employed vegetable poi-

Confined to vegetable poisons.

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[*] Philos. Trans. for 1811, p 178.

sons only. Of these I have selected such, as are very active and certain in producing their effects, believing that, on this account, the exact nature of those effects would be more readily ascertained. The principal objects, which I have kept in view, have been to determine, on which of the vital organs the poison employed exercises its primary influence, and through what medium that organ becomes affected. I have also endeavoured to ascertain by what means the fatal consequences of some poisons may be prevented. With some of the conclusions, which I have ventured to draw, so far as I know, we were not before acquainted; and others of them, though not entirely new, had not been previously established by satisfactory experiments.

1st applied to the tongue, or taken internally.

I shall relate first those experiments, in which poisons were applied internally; that is to the mucous membranes of the tongue or alimentary canal, and afterward those, in which poisons were applied to wounded surfaces.

II. *Experiments with Poisons applied to the Tongue or alimentary Canal.*

Experiments with Alcohol.

Effects of alcohol.

When spirits are taken into the stomach, in a certain quantity, they produce that kind of delirium, which constitutes intoxication; when taken in a larger quantity, it is well known, that they destroy life altogether, and this in the course of a very short space of time. Intoxication is a derangement of the functions of the mind; and, as these are in some way connected with those of the brain, it seems probable, that it is by acting on this organ, that spirits, when taken into the stomach, occasion death. In order to ascertain how far this conclusion is just, I made the following experiments*.

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* I am indebted to Dr. E. N. Bancroft for his assistance in many of the experiments, which I am about to detail. Mr W. Brande lent me his assistance in the greater part of those which were made. I have been farther assisted by Mr. Broughton, Mr. R. Rawlins, and Mr R. Gatcombe, and by several other gentlemen.

Experiment 1.

Experiment 1. I poured two drachms of proof spirits down the œsophagus of a cat. Instantly he struggled violently; then lay on one side, perfectly motionless and insensible; the breathing was laboured and stertorous, and the pulsations of the heart were very frequent. He continued in this state for seven or eight minutes; then began to recover; the respirations became easier, and presently he stood up, and was able to walk.

Experiment 2.

Exp. 2. I injected an ounce and a half of proof spirits into the stomach of a large full-grown rabbit, by means of an elastic gum tube passed down the œsophagus. The same symptoms took place as in the last experiment; but the animal did not begin to recover from the state of insensibility, until forty minutes had elapsed from the time of the injection.

Experiment 3.

Exp. 3. Seven drachms of proof spirits were injected into the stomach of a younger rabbit. Two minutes afterward, he evidently was affected by the spirits, and in three minutes more he lay on one side motionless and insensible. The pupils of the eyes were perfectly dilated; there were occasional slight convulsive motions of the extremities; the respiration was laborious, it was gradually performed at longer and longer intervals, and, at the end of an hour and fifteen minutes had entirely ceased. Two minutes after the animal was apparently dead, I opened into the thorax, and found the heart acting with moderate force and frequency, circulating dark coloured blood. I introduced a tube into the trachea, and produced artificial respiration by inflating the lungs, and found that by these means the action of the heart might be kept up to the natural standard, as in an animal from whom the head is removed.

Experiment 4.

Exp. 4. I injected into the stomach of a rabbit two ounces of proof spirits. The injection was scarcely completed, when the animal became perfectly insensible. Precisely the same symptoms took place as in the last experiment; and at the end of twenty-seven minutes, from the time of the injection, the rabbit was apparently dead; but on examining the thorax the heart was found still acting, as in the last experiment.

It

The brain not directly necessary to the action of the heart.

It has been shown by Mr. Bichat, and the observation has been confirmed by some experiments, which I have lately had the honour of communicating to this learned Society*, that the brain is not directly necessary to the action of the heart; and that, when the functions of the brain are destroyed, the heart continues to contract for some time afterward, and then ceases only in consequence of the suspension of respiration, which is under the influence of the brain.

Spirits act on the brain,

It would appear from the experiments, which I have just detailed, that the symptoms, produced by a large quantity of spirits taken into the stomach, arise entirely from disturbance of the functions of the brain. The complete insensibility to external impressions, the dilatation of the pupils of the eyes, and the loss of motion, indicate, that the functions of this organ are suspended; respiration, which is under its influence, is ill performed, and at last altogether ceases; while the heart, to the action of which the brain is not directly necessary, continues to contract, circulating dark coloured blood for some time afterward.

and affect it in the same way as external injuries.

There is a striking analogy between the symptoms arising from spirits taken internally, and those produced by injuries of the brain.

Concussion of the brain, which may be considered as the slightest degree of injury, occasions a state of mind resembling intoxication; and the resemblance in some instances is so complete, that the most accurate observer cannot form a diagnosis, except from the history of the case. Pressure on the brain, which is a more severe injury than concussion, produces loss of motion, insensibility, dilatation of the pupils; respiration becomes laboured and stertorous, is performed at long intervals, and at last altogether ceases, and the patient dies.

Do they act by absorption, or sympathy?

It forms an interesting matter of inquiry, whether spirits, when taken into the stomach, produce their effects on the brain by being absorbed into the circulation, or in consequence of the sympathy, that exists between these organs by means of the nerves. The following circumstances lead me to conclude, that they act in the last of these two ways.

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* See Journal, vol. XXIX, p. 359.

1. In

Most probably the latter.

1. In experiments where animals have been killed by the injection of spirits into the stomach, I have found this organ to bear the marks of great inflammation, but never found any preternatural appearance whatever in the brain. 2. The effects of spirits taken into the stomach in the last experiment were so instantaneous, that it appears impossible that absorption should have taken place before they were produced. 3. A person who is intoxicated, frequently becomes suddenly sober after vomiting. 4. In the experiments, which I have just related, I mixed tincture of rhubarb with the spirits, knowing from the experiments of Mr. Home and Mr. William Brande, that this, when absorbed into the circulation, was readily separated from the blood by the kidneys, and that very small quantities might be detected in the urine by the addition of potash; but, though I never failed to find urine in the bladder, I never detected rhubarb in it.

The including the termination of the thoracic duct in a ligature does not prevent spirits, when taken into the stomach, from producing their usual effects on the nervous system; but subsequent observations, which Mr. Home has already communicated to this Society[*], have shown, that no conclusion can be drawn from this experiment.

A poison may act without entering the circulation.

That a poison may affect a distant organ, through the medium of the nerves, without entering the circulation, is proved by the well-known circumstance of solution of the extract of *belladonna*, when applied to the tunica conjunctiva of the eye, occasioning dilatation of the pupil of the same eye, though no other part of the system is affected.

Supposed capable of producing death this way.

It has been formerly supposed by Dr. Mead and other physiologists, that a poison may produce death by acting on the extremities of the nerves of the stomach and intestines, without being absorbed into the circulation. That it should by these means be capable of affecting the brain is not to be wondered at, when we consider the numerous and various sympathies between this organ and the alimentary canal, evidently independent of any other communication than the nerves.

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[*] See p. 178, of our present vol.

Experiments

Experiments with the Essential Oil of Bitter Almonds[*].

Effects of oil of bitter almonds.
Experiment 5.

Experiment 5. One drop of the essential oil of bitter almonds was applied to the tongue of a young cat. She was instantly seized with violent convulsions; then lay on one side motionless, insensible, breathing in a hurried manner; the respirations became laboured, took place at longer and longer intervals, and at the end of five minutes, from the application of the poison, had entirely ceased, and the animal was apparently dead; but, on opening the thorax, the heart was found acting regularly eighty times in a minute, circulating dark coloured blood, and it continued to act for six or seven minutes afterward.

Experiment 6.

Exp. 6. I injected into the rectum of a cat half an ounce of water, with two drops of the essential oil. In two minutes afterward, he was affected with symptoms similar to those, which occurred in the last experiment; and at the end of five minutes, from the injection of the poison, he was apparently dead. Two minutes after apparent death, the heart was found acting eighty times in a minute. On dissection, no preternatural appearances were found either in the internal membrane of the rectum, or the brain.

Appears to act on the brain.

The symptoms produced by this poison, and the circumstance of the heart continuing to contract after apparent death, lead to the conclusion, that it occasions death by disturbing the functions of the brain.

Effects of a drop applied to the author's tongue.

While engaged in these last experiments, I dipped the blunt end of a probe into the essential oil, and applied it to my tongue, meaning to taste it; and having no suspicion, that so small a quantity could produce any of its specific effects on the nervous system; but scarcely had I applied it, when I experienced a very remarkable and unpleasant sensation, which I referred chiefly to the epigastric region, but the exact nature of which I cannot describe, because I know nothing precisely similar to it. At the same time there was a sense of weakness in my limbs, as if I had not the command of my muscles, and I thought that I was about to fall. However, these sensations were momentary, and I experienced no inconvenience whatever afterward.

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[*] The essential oil of bitter almonds does not appear to differ from the essential oil of laurel. I was furnished with a quantity of it, first by my friend Mr. William Brande, and afterward by Mr. Cooke of Southampton street.

I afterward applied a more minute quantity of the essential oil to my tongue several times, without experiencing from it any disagreeable effects; but on applying a larger quantity, I was affected with the same momentary sensations as in the former instance, and there was a recurrence of them in three or four seconds after the first attack had subsided.

Acts through the medium of the nerves.

From the instantaneousness, with which the effects are produced; and from its acting more speedily when applied to the tongue, than when injected into the intestine, though the latter presents a better absorbing surface; we may conclude, that this poison acts on the brain through the medium of the nerves, without being absorbed into the circulation.

Experiment with the Juice of the Leaves of Aconite.

Effects of the juice of aconite. Experiment 7.

Exp. 7. An ounce of this juice was injected into the rectum of a cat. Three minutes afterward he voided what appeared to be nearly the whole of the injection; he then stood for some minutes perfectly motionless, with his legs drawn together; at the end of nine minutes, from the time of the injection, he retched and vomited; then attempted to walk, but faltered and fell at every step, as if from giddiness. At the end of thirteen minutes, he lay on one side insensible, motionless, except some slight convulsive motions of the limbs. The respiration became slow and laboured; and at forty-seven minutes from the time of the injection, he was apparently dead. One minute and a half afterward, the heart was found contracting regularly one hundred times in a minute.

It acts in a similar way.

It appears from this experiment, that the juice of aconite, when injected into the intestine, occasions death by destroying the functions of the brain. From the analogy of other poisons, it is rendered probable, that it acts on the brain through the medium of the nerves, without being absorbed into the circulation. This opinion is confirmed by the following circumstance; if a small quantity of the leaf of aconite is chewed, it occasions a remarkable sense of

Effects of chewing the leaf.

numbness of the lips and gums, which does not subside for two or three hours.

Experiments with the Infusion of Tobacco.

Effect of tobacco.
Experiment 8.

Exp. 8. Four ounces of infusion of tobacco were injected into the rectum of a dog. Four minutes afterward he retched, but did not vomit; he then became faint, and lay motionless on one side; at the end of nine minutes from the time of the injection, the heart could not be felt; he gasped for breath at long intervals; and in another minute there was no appearance whatever of life. I immediately laid open the cavities of the thorax and abdomen. The heart was much distended, and had entirely ceased to contract; there was no peristaltic motion of the intestines.

Experiment 9.

Exp. 9. An ounce of very strong infusion of tobacco was injected into the rectum of a cat. Symptoms were produced similar to these, which occurred in the last experiment, and the animal died at the end of seven minutes from the time of the injection. On opening the thorax immediately after death, the heart was found extremely distended, and to have entirely ceased acting, with the exception of a slight tremulous motion of the auricles.

Exp 10.

Exp. 10. Three ounces of infusion of tobacco were injected into the rectum of a dog. He was affected with symptoms similar to those in the former experiments, and died at the end of ten minutes. On opening the thorax immediately after death, I found the heart much distended, and to have entirely ceased contracting.

Exp. 11.

Exp. 11. Three ounces of infusion of tobacco were injected into the rectum of a dog. Immediately there took place tremulous contractions of the voluntary muscles. Five minutes afterward the injection was repeated in the same quantity. The dog then was sick, and threw up some of the infusion, with other matter, from the stomach; he became faint, and died ten minutes after the second injection. Immediately after respiration had ceased, I opened the thorax, and found the heart extremely distended, and without any evident contraction, except of the appendix of the right auricle, which every now and then contracted in a slight degree. I divided the pericardium on the right side.

In consequence of the extreme distension of the heart, this could not be done without irritating the fibres with the point of the scalpel. Immediately both auricles and ventricles began to contract with considerable force, so as to restore the circulation. Artificial respiration was produced, and the circulation was kept up for more than half an hour, beyond which time the experiment was not continued.

It destroys the action of the heart,

We may conclude from these experiments, that the effect of the infusion of tobacco, when injected into the intestine of a living animal, is to destroy the action of the heart, stopping the circulation and producing syncope. It appeared to me, that the action of the heart ceased, even before the animal had ceased to respire; and this was confirmed by another experiment, in which, in a dog killed by the infusion of tobacco, I found the cavities of the left side of the heart to contain scarlet blood, while in those of the right side the blood was dark coloured. This poison therefore differs materially from alcohol, the essential oil of almonds, and the juice of aconite, which have no direct influence on the action of the heart. The infusion of tobacco renders the heart insensible to the stimulus of the blood, but it does not altogether destroy the power of muscular contraction, since the heart resumed its action in one instance on the division of the pericardium; and I have found, that the voluntary muscles of an animal killed by this poison are as readily stimulated to contract by the influence of the Voltaic battery, as if it had been killed in any other manner. At the same time, however, that the infusion of tobacco destroys the action of the heart, it appears to destroy also the functions of the brain, since these did not return in the last experiment; although the circulation was restored, and kept up by artificial respiration.

and also of the brain.

Its absorption by the blood questionable.

Since there is no direct communication between the intestinal canal and the heart, I was at first induced to suppose, that the latter becomes affected in consequence of the infusion being conveyed into the blood by absorption. Some circumstances in the following experiment have since led me to doubt, whether this is the case.

Exp. 12.

Exp. 12. In a dog, whose head was removed, I kept up the circulation by means of artificial respiration, in the manner already described in the account of some experiments, which I lately communicated to this Society. I then injected into the stomach and intestines nine ounces of infusion of tobacco. At the time of the injection, the body of the animal lay perfectly quiet and motionless on the table; the heart acted regularly one hundred times in a minute. Ten minutes afterward the pulse rose to one hundred and forty in a minute; the peristaltic motion of the intestines was much increased, and the voluntary muscles in every part of the body were thrown into repeated and violent spasmodic action. The joints of the extremities were alternately bent and extended; the muscles of the spine, abdomen, and tail alternately relaxed and contracted, so as to turn the whole animal from one side to the other. I have observed, in other instances, spasmodic actions of the muscles, where the circulation was kept up by artificial respiration, after the removal of the head; but not at all to be compared, either in strength or frequency, with those, which took place on this occasion. I made pressure on the abdominal aorta for more than a minute, so as to obstruct the circulation of the blood in the lower extremities; but the muscular contractions were not lessened in consequence. Half an hour after the injection of the infusion, the artificial respiration was discontinued. The heart continued to act, circulating dark coloured blood; the muscular contractions continued, but gradually diminished in strength and frequency. I tied a ligature round the vessels at the base of the heart, so as to stop the circulation, nevertheless the muscular contractions still continued, though less frequent and forcible than before, and some minutes elapsed before they entirely ceased.

Remarks on the phenomena.

In this experiment, the disposition to contraction in the muscles was very much increased, instead of being diminished, as in those just related. If the infusion of tobacco influences the heart from being absorbed into the blood, and thus coming into actual contact with its fibres, there is no evident reason, why the removal of the brain, and the employment of artificial respiration, should occasion so material a difference in its effects. If the contractions of the voluntary muscles had depended on the infusion circulating with the blood, it is reasonable to suppose, that the pressure on the aorta would have occasioned some diminution of them, and that the complete obstruction of the circulation would have caused them to cease altogether.

Appears to act on the nervous system,

From these considerations I am induced, on the whole, to believe, that the infusion of tobacco, when injected into the intestines, influences the heart through the medium of the nervous system; but I have not been able to devise any experiment, by which the truth or fallacy of this opinion might be put beyond the reach of doubt.

affecting it very powerfully.

It appears remarkable, that the brain and nervous system, although not necessary to the action of the heart, should, while under the influence of the infusion of tobacco, be capable of influencing this organ so as to stop its action; but this is analogous to what we see occur in consequence of violent emotions of the mind. Those states of the nervous system, which accompany the passions of joy, fear, or anger, when existing in a moderate degree, render the heart more sensible to the stimulus of the blood, and increase the frequency of its contractions; while, when the same passions exist in a greater degree, the heart is rendered altogether insensible to the stimulus of the blood, and syncope ensues.

*Experiments with the Empyreumatic Oil of Tobacco**.

Effects of empyreumatic oil of tobacco. Exp. 13.

Exp. 13. Less than a drop of this oil was applied to the tongue of a young cat. Instantly violent convulsions took place in all the muscles, and the respirations became very frequent. In five minutes after the application, she lay on one side insensible, with slight spasmodic actions of the muscles. At the end of eleven minutes, she retched, but did not vomit. In a quarter of an hour, she appeared to be recovering. I repeated the application of the poison, and she was again seized with violent convulsions, and became insensible, breathing at long intervals, and in two minutes from the second application respiration had entirely ceased, and she was apparently dead. On opening the thorax, I found the heart acting with regularity and strength, circulating dark-coloured blood. I introduced a tube into the trachea, and produced artificial respiration; the contractions of the heart became augmented in force and frequency, and there was no evident diminution in six or seven minutes, during which the artificial respiration was continued.

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* I was furnished with the empyreumatic oil of tobacco by Mr. W. Brande. It may be procured by subjecting the leaves of tobacco to distillation in a heat above that of boiling water: a quantity of watery fluid comes over, on the surface of which is a thin film of unctuous substance.

On dissection, nothing remarkable was found in the appearance of the tongue or brain.

Acted like the essential oil of almonds.

The symptoms and mode of death, in this experiment, did not essentially differ from those produced by the essential oil of almonds. I was surprised to find the effects of the empyreumatic oil so entirely different from those of the infusion of tobacco. Supposing that this difference might arise from the poison being more concentrated in the oil than in the infusion, I made the following experiments.

Exp. 14.

Exp. 14. A drop of the oil of tobacco was suspended in an ounce and a half of water by means of mucilage of gum arabic, and the whole was injected into the rectum of a dog. In two minutes afterward he became faint, retched, but did not vomit. He appeared to be recovering from this state, and in twenty-five minutes after the first injection, it was repeated in the same quantity. He was then seized with symptoms similar to those in the last experiment, and in two minutes and a half he was apparently dead.

Two minutes after apparent death, on the thorax being opened into, the heart was found acting regularly one hundred times in a minute, and it continued acting for several minutes.

Exp. 15.

Exp. 15. A drop of the empyreumatic oil of tobacco with an ounce of water was injected into the rectum of a cat. The symptoms produced were in essential circumstances similar to those, which occurred in the last experiment. The animal was apparently dead in five minutes after the injection, and the heart continued to contract for several minutes afterward.

We may conclude from these experiments, that the empyreumatic oil of tobacco, whether applied to the tongue, or injected into the intestine, does not stop the action of the heart and induce syncope, like the infusion of tobacco; but that it occasions death by destroying the functions of the brain, without directly acting on the circulation. In other words, its effects are similar to those of alcohol, the juice of aconite, and the essential oil of almonds.

(To be concluded in our next.)

XII.

Analysis of a Chinese Gong-gong: by Mr. Klaproth[*].

Sonorousness of bell metal.

AMONG sonorous instruments the composition of copper with tin gives the loudest sound. Bells, we know, are composed of this alloy. The celebrated bell of Pekin, the largest in the World, which is twenty feet in diameter, and sixteen inches thick, is no doubt cast of it.

The Chinese gong.

The Chinese frequently use another kind of bells too, which are not cast, but hammered out. These instruments, called *gongs*[†], are not shaped like a common bell, but like a shield with the edge turned up: and give an astonishing sound when struck. Barrow, in his voyage to China, says of these instruments; that they are like flat pots, or rather potlids; that they are struck with a stick wrapped round with leather; and that they are supposed to be formed of copper, tin, and bismuth.

Analysis of it.

The thickness of this alloy is about that of the back of a knife; its colour is a bronze yellow; and its spec. grav. 8·815.

A hundred and fifty grains were heated with nitric acid; and 42 grs of oxide of tin separated; answering to 33 grs of metallic tin.

Into the filtered liquor sulphuric acid was poured, and the mixture was evaporated to dryness. The residuum being dissolved in water, iron precipitated from it 117 grs of copper.

Its composition.

The gong therefore is composed of Copper ······· 78
Tin ·········· 22

100.

Cause of its loud sound.

The property of emitting a sound that can be heard so far depends on the mutual penetration of the metals, and the greater density of the alloy, which is farther increased by hammering. Perhaps too the form of the instrument contributes to this.

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[*] Ann de Chim. vol LXXV, p. 322.

[†] *Tshoung*, in the Chinese language, signifies a bell,

XIII.

METEOROLOGICAL JOURNAL.

		PRESSURE.			TEMPERATURE.					
	Wind	Max.	Min.	Med.	Max.	Min	Med.	Evap.	Rain	
10th Mo.										
Oct. 9	S W	30·00	29·99	29·995	67	54	60·5	—		☽
10	S W	29·99	29·77	29·880	63	57	60·0	—		
11	S W	29·77	29·60	29·685	65	51	58·0	—		
12	S	29·80	29·60	29·700	62	48	55·0	·40	·08	
13	S W	29·90	29·86	29·880	62	49	55·5	—	·03	
14	S	29·86	29·81	29·835	63	51	57·0	—	·01	
15	S	29·76	29·75	29·755	73	53	63·0	·33		
16	S	30·03	29·76	29·895	70	55	62·5	—		
17	Var.	30·10	30·03	30·065	71	47	59·0	—		●
18	S W	30·16	30·10	30·130	68	50	59·0	—	·11	
19	Var.	30·21	30·18	30·195	65	49	57·0	·20		
20	S W	30·05	29·96	30·005	64	55	59·5	—		
21	S W	29·96	29·50	29·730	65	56	60·5	·20		
22	S	29·52	29·46	29·490	64	50	57·0	—	·15	
23	S W	29·50	29·48	29·490	60	49	54·5	—		
24	Var.	29·48	29·35	29·415	57	42	49·5	·20	·08	
25	S	29·35	28·65	29·000	53	38	45·5	—	·18	☾
26	Var.	28·80	28·65	28·725	54	41	47·5	·15	·32	
27	S E	28·84	28·81	28·825	56	43	49·5	—	·11	
28	Var.	28·84	28·80	28·820	56	41	48·5	·13	·44	
29	S W	29·05	29·00	29·025	55	43	49·0	·02	.18	
30	Var.	29·55	29·00	29·275	58	43	50·5	—	·14	
31	W	29·77	29·68	29·725	59	48	53·5	·15	·14	○
11th Mo.										
Nov. 1	S W	29·68	29·62	29·650	62	57	59·5	—	·11	
2	S W	29·58	29·50	29·540	62	53	57·5	·34	·14	
3	S W	29·70	29·60	29·650	58	48	53·0	—	·08	
4	W	29·98	29·80	29·890	60	42	51·0	·17	—	
5	S W	29·89	29·83	29·860	56	43	49·5	—	·25	
6	S W	29·83	29·52	29·675	53	45	49·0	·13	·50	
		30·21	28·65	29·614	73	38	54·86	2·44	3·05	

N. B. The observations in each line of the Table apply to a period of twenty four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Tenth Month. 12. Windy: wet evening. 13. Much wind. 14. A shower before nine a. m. at which time occurred the max. of temp. 15 Much dew on the grass: serene day: twilight milky, with converging streaks of red. 16 a. m. Much dew: a mist on the river: the smoke of the city remarkably depressed, and sounds unusually strong from thence: some thunder clouds appeared and passed to E. 17. *Cumulus* clouds surmounted with *cirrostratus*, and *cirri* above. 18 A very wet mist a. m. wind N.W.: at two p. m. cloudy; very moist air, the *dew point* (or temperature at which a body colder than the air condenses water from it) being 63°: about sunset, at temp. 63°, I found dew just beginning to be deposited on the grass: it rained hard about five next morning. 19. a. m. Misty, small rain: p. m clear: evening, *cirri* very elevated, and long coloured red; a *stratus* forming. 20. Misty: then overcast: the wind, which had been E., veering by S.: abundance of *gossamer.* A quicken-tree (*sorbus aucuparia*) exhibits a new set of leaves and blossoms along with the ripe berries: 21. Gray morning, with little dew and a strong breeze. 22. a. m. Dew scarce perceptible: wind veers to S., a breeze: p. m. very cloudy, with showers: much wind at night. 24. At mid day a drizzling rain, during which the vane turned to E. 25. Clear, fine day: wind veered to S.: at sunset *nimbi* and *cirrostrati* in S. W.: heavy shower by eleven p. m. 26. Showery: a fine rainbow at ten a. m. 27. a m. *Nimbi* in different quarters, mixed with *cumulus* and *cirrostratus*, beneath large plumose *cirrus* clouds. 28. a. m. Clear, much dew, *nimbi* forming amidst various clouds: vane at N. E: p m. a shower in the S., during which appeared, for a short time, a numerous flight of swallows: they had been last observed on the 15th: the wind returned by S. to N. W. with much cloud and rain. 30. At nine a. m. the rain intermitting, the highest and most considerable mass of clouds was moving from W. an intermediate portion from S., and the wind below fresh at E.: in this state of things sounds came very freely from the westward, and by eleven the wind was S. W.: at three p. m. distinct *nimbi* and a bright bow: showery at night, with a lunar halo. 31. a. m. Clear: the sun and moon appeared red on the horizon: at night, the wind being S. sounds came loud from the W.

Eleventh Month. 1. a. m. Much cloud: wind fresh at S. W. 2. As yesterday: stormy at night 3. A rainbow at eight a. m. 4. a. m. *Nimbi* to windward. at sunset, the dense clouds in the E. finely coloured: rainbow: wind W. 5. a. m. Stormy: p. m. wet. 6. Cloudy, showery: evening, abundance of *cirrostratus*: a wet night.

RESULTS.

Barometer: highest observation 30·21 inches; lowest 28·65 inches; range 1·56 inches.
Mean of the period 29·614 inches.

Thermometer: highest observation 73°; lowest 38°; range 35°.
Mean of the period 54·86°.

Evaporation 2·44 inches. Rain 3·05 inches.

Wind with little exception S. W. and S. The fore part of the period changeable; the latter wet, without the usual intervening frosty nights.

L. HOWARD.

PLAISTOW, *Eleventh Mo.* 20, 1811.

XIV.

Chemical Examination of the yellow Resin of the Xanthorrhœa hastilis, and of the resinous Cement employed by the Savages of New Holland to fix the Stone of their Hatchets: by Mr. A. LAUGIER.*

Yellow gum from Botany Bay.

THE following remarks on the resin of the xanthorrhœa, and the tree that produces it, which Mr. Peron has been so obliging as to communicate to me, will form a very suitable introduction to the experiments I shall relate, and enhance their value.

The tree.

"The resin in question," says Mr. Peron, "exudes naturally from the bark of a tree peculiar to New Holland, and of which Dr. Smith has made a new genus, under the name of *xanthorrhœa hastilis*; thus intending to express in one term the colour of the resin of this strange tree, and in the other the use, which the natives make of its shoots for their spears.

Its name not unexceptionable.

"It must be observed however, that Dr. Smith's generic name is not strictly accurate; as the resin is very frequently brown, red like dragon's blood, green, &c. Hence the different names of yellow, red, green, &c., gum plant, or gum tree, given almost indiscriminately to the xanthorrhœa by the English at Port Jackson. Whether these varieties of colour indicate so many species, or varieties, of the tree that produces them; or depend merely on the age or other circumstances of the individual tree; has not yet been ascertained.

Probably several species.

"Hitherto botanists have admitted only one species of xanthorrhœa, the hastilis, just mentioned: but as trees of this kind are found throughout the various parts of New Holland, an extent of country equal to all Europe, it is very probable, that several species exist.

Phillip's description and figure indifferent.

"Governor Phillip, in his voyage to Botany Bay, p. 60, and plate to p. 149, has given an incomplete description of the xanthorrhœa; and a figure, which, though not very carefully executed, is sufficient to afford an idea of this extraordinary tree.

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* Ann. de Chim. vol. LXXVI, p 265.

"It

Its soil, and growth.

" It is particularly abundant at Geographer's bay, Leuwin's Land, and in the environs of Botany Bay; and appears to prefer a sandy and barren soil. The shoots, which the savages use for their spears, extend to the length of three, four, or even five yards; and are nearly of the same size, which is scarcely equal to that of the thumb, throughout their whole length.

Produces a sweet juice

" Each of these shoots terminates in a kind of spike, or ear, of a larger size, and from fifteen to twenty four inches long; from the surface of which exudes a kind of viscous liquid, of a pleasant sacharine taste, and a strong aromatic smell. The savages are very fond of it: and I found it, on tasting it, to be as I have described. To procure these tops of the xanthorrhœa, the natives have recourse to their clubs [*casse-tête*], which they throw with such strength and skill, that they are sure to cut off the ear at what length they please at the first stroke.

The resin.

" The resin flows naturally from the trunk of the tree, making its way through the bark. The portion of the stem, that is buried in the sand, appears to furnish the greater part; at least large pieces are found in the sand, apparently still adhering to the bark. Some of these pieces are remarkable for the perfect regularity of their spherical form.

Its uses.

" The English employ this resin against dysentery, for which they esteem it an excellent medicine. The savages use it for many domestic purposes, and particularly for cementing the points of their spears to the shaft. With this substance too they prepare the celebrated instrument, that serves to discharge their spears; also their fishing implements, their stone hatchets, &c. They likewise employ it to unite the lips of wounds, however large or dangerous they may be; and I have seen some healed in this way by the first intention, that have appeared to me truly extraordinary.

The wood fragrant when burned.

" The wood of the xanthorrhœa, when burned, emits a smell which is very pleasant, at a little distance from the fire, but seemed to me too powerful if inhaled nearer. Such indeed is the odoriferous strength of this wood, that you may sometimes discover a party of savages more than a quarter of a league [half a mile] distant merely by the smell it emits in burning.

Probably the eagle wood of India.

"Mr, Martin-Moncan, formerly agent of the French government to Hyder Ali Khan, told me, on seeing a piece of the xanthorrhœa, and smelling to it, that it very much resembled the celebrated eagle wood, which fetches such a high price in India, and the country of which is hitherto unknown to Europeans." Mr. Martin-Moncan considered it as by no means impossible, that the Malays, who in fact have long had a commercial intercourse with New Holland, visit its coast to procure the wood of the xanthorrhœa, which he believes to be the eagle wood itself."

Physical properties of the yellow resin.

The resin of the xanthorrhœa is friable and easily separates into scales before the nail. Its fracture is shining and compact. It has a yellow colour, and a very pleasant balsamic smell, resembling that of poplar buds. When rubbed in a mortar, it clots, and adheres to it strongly. It is rendered very perceptibly electric by friction. The paper on which it has been put when powdered retains enough of it to acquire a deep yellow colour, which cannot be removed.

Action of heat on it.

Exposed to a gentle heat, it melts, swells up, gives out a considerable portion of aqueous vapour, diminishes in bulk, and acquires a brownish-red colour inclining to purple. Placed on burning coals, it rises in dense fumes, very pungent, and so strongly aromatic as to be disagreeable; and soon after it flames, swells up considerably, and leaves a very bulky and very light coally residuum.

Action of alcohol.

As this substance does not mix with water, and imparts to it no colour, acting in this respect as a resin, I employed for analysing it alcohol at 40° [sp. grav. 0·817], which dissolved it with the greatest facility, and without the assistance of heat. Nothing was left undissolved but 0·07 of an insipid, grumous substance, resembling a gum, and particularly that which is called in the shops gum of Bassora, for it is neither soluble nor diffusible in water, it is only softened and swelled up by the action of this fluid when boiling.

The tincture partly precipitated by water.

The alcoholic solution when filtered has a reddish colour, and is remarkable for its limpidity and pleasant smell. It may be kept several months without undergoing any alteration.

tion. Water renders it turbid, and occasions a precipitate, but a portion of the resin remains suspended, without being separable either by heat or standing; so that the mixture resembles a solution of gum-resin. If however it be heated long enough to evaporate the alcohol, and about three fourths of the liquid, almost all the resin is deposited on the bottom and sides of the vessel, and the portion more minutely divided unites on cooling into little tufts of a lemon colour. The mixture in this state has a more pleasant and delicate smell than the resin itself, and some compare it to that of storax.

Benzoic acid obtained from the water.

The water separated from the resin was still turbid, or a little coloured, and reddened vegetable blues. In order to fix the acid it contained, I had recourse to the process, which I employed with success in my analysis of the substance found in the grotto of Arc, and of castor, to obtain from it the benzoic acid: I added a few drops of caustic potash, and I evaporated to dryness. The residuum, which resembled a kind of brown red extract, was distilled with a little sulphuric acid diluted with water, and toward the end of the process I obtained a few small crystals, which had the characteristics of benzoic acid.

These small crystals I diffused in the acid and aromatic water in the receiver, and supersaturated the mixture with lime quenched in the air. After evaporating to dryness, I poured on the residuum a small quantity of cold water, to take up the benzoate of lime, and separate it from the sulphate and carbonate of this base, which were mingled with it. Into the filtered and concentrated liquor I poured muriatic acid, which produced in it a slight precipitate of benzoic acid in the form of small granular crystals.

Readier mode of obtaining this acid.

But I found, that the most simple and ready mode f distinguishing the presence of this acid in the yellow resin as, to expose this substance to a heat sufficient to keep it fusion. I introduced the powdered resin into a very dry ssel, which I placed on a sand-heat; and as soon as the in was melted aqueous vapours first rose, and soon after ite fumes, which condensed on the sides in small shining [illegible], exhibiting all the characteristics of benzoic acid.

s the acid is expelled, the resin first swells up: after which it collapses, and diminishes in bulk. In this state it is of a deep brown colour, which appears purplish when placed between the eye and the light.

The alcoholic solution also yields a few crystals of benzoic acid by distillation to dryness, though not so easily. The alcohol distilled from it reddens litmus paper, which shows, that it probably carries off a portion of the same acid.

The resin distilled with water

yielded a hot and fragrant oil.

I introduced 30 grains of the yellow resin into a retort with four ounces of distilled water, fitted to it a receiver, and distilled on a sand-heat. The water, that passed into the receiver, was turbid; on account of the suspension of a certain quantity of essential oil, several drops of which collected on the surface. The water thus mixed with oil had an extremely pleasant smell. The extremity of the beak of the retort was soiled with this oil, which had an acrid and burning taste nearly like that of the oil of cloves. When the matter remaining in the retort was dry, white fumes arose, that condensed in the head of the retort in small and very white crystals, which powerfully reddened litmus paper, and had the strong and pleasant smell of benzoic acid.

The oil obtained from the tincture.

The essential oil of the yellow resin may be obtained also by distilling the alcoholic solution: the alcohol, that passes over into the receiver, being insensibly impregnated with it; so that the evaporation of this liquid by a gentle heat is sufficient to procure this acrid and pleasant substance.

The resin forms a soap with alkalis or lime.

Caustic alkalis, or lime, placed in contact with the yellow resin, immediately assume a deep yellow colour, without the assistance of heat; and dissolve the resin completely, if they be employed in sufficient quantity. The solution froths when shaken, like that of soap, and lets fall a yellowish white precipitate on the addition of an acid.

The benzoic acid not obtainable from this.

I had hoped, that this solvent action of the alkalis would furnish me with an easy mode of separating the benzoic acid from the resin; but several trials convinced me of th impossibility of succeeding. It appears, that this acid fal down at the same time as the resin, the moment anoth acid is added to the mixture.

The resin

Thirty grains of the yellow resin in powder being heat in a retort with six times the weight of nitric acid, a con

dense

treated with nitric acid.

derable evolution of nitrous gas was produced, and the resin was completely dissolved. The liquor remaining in the retort deposited by cooling a crystalline substance; and both the mother-water and the crystals were of a deep yellow colour, a very bitter taste, and a smell of bitter almonds. A portion of the mother-water being saturated with potash, it did not emit any sensible ammoniacal smell; but being mixed with a solution of sulphate of iron, and supersaturated with concentrated sulphuric acid, it let fall in the course of the night a considerable quantity of Prussian blue. Another portion of the same mother-water yielded on evaporation thin crystals several lines square, which might be known for oxalic acid. Their solution precipitated lime-water, and the calcareous salts.

Conclusions.

From the experiments I have related it appears, that the yellow substance, which flows from the xanthorrhœa, is composed of a large portion of resin, combined with a few hundredths of a kind of spongy gum, insoluble in water, of benzoic acid, and of a very acrid, yellowish volatile oil, very pleasant to the smell.

It is properly a balsam.

The yellowish substance of the xanthorrhœa then cannot be considered as a resin, properly so called; since it differs from resins in containing benzoic acid, to which it is indebted at least in some measure for its pleasant smell; and on this account it seems to belong rather to the balsams, than to the resins.

Resembles propolis.

What struck me most in the examination of this yellow substance is its resemblance to that matter, which the bees employ for stopping cracks in their hives, and to which the name of propolis has been given.

This resinous, odoriferous matter, when separated from the wax, by which its properties are concealed, exhibits the characters of the yellow substance; and, if subjected to the same processes, comports itself in the same manner.

The resin on poplar buds similar.

It is considered by naturalists as ascertained almost to a demonstration, that the resinous matter, which covers the buds of poplar trees, and preserves them from moisture, is that which the bees so carefully collect, to form their propolis. The smell of this matter, which is precisely the same with that of the propolis, strongly supports this opinion.

The smell of the yellow substance too is similar to that of the poplar buds : and, if we cannot hence infer its perfect identity with propolis, it is at least certain, that the difference between them is too trifling to admit the supposition, that bees could not employ the yellow substance for the same purpose. This conjecture, however, might easily be verified in countries where the tree that produces it so abundantly grows.

A cement of great strength made with this resin.

The resin I have just analysed enters into the composition of a cement, which the natives of New Holland employ for fixing the stone of their hatchets to the handle, and for securing the points of their spears. This cement is capable of acquiring such hardness, that the hardest substances cannot separate it, or even loosen the stone fastened by it. Its colour is a deep brown; and on rubbing it emits a fragrant smell, which does not differ from that of the yellow resin.

This cement analysed.

I satisfied myself of the complete identity of this cement with the yellow resin by examining a sufficient quantity of it, taken from a hatchet brought home by Mr. Peron, and which her majesty, the empress Josephine, deigned to accept from that navigator, as a valuable proof of the industry of the natives of Nuyts's Land.

A hundred parts of the brown powder furnished by the cement were digested in alcohol at 40° [sp. gr. 0·817]. Two portions of this liquid added in succession were sufficient to take up all the resin, that the cement contained. What remained after the action of the alcohol was nothing but a blackish gray powder, without smell or taste. The weight of this residuum was 51 parts, so that the alcohol had taken up 49.

The alcoholic solution had a deep red colour, and w exactly similar to that obtained by macerating in the sa menstruum the yellow resin, after it had been melted a turned brown by heat. On evaporation it yielded a d resin, which had all the characters of the resin of the x-thorrhœa.

On the 51 parts not dissolved by the alcohol I boile to dryness a small quantity of nitric acid, which caused th e-siduum to acquire a redness like that of oxide of iron, nd I treated this residuum with muriatic acid. After th ac-

ion

tion of this acid, the residuum, being 37 parts, was a white, dry powder, rough to the finger, and resembling fine sand.

Ammonia, poured into the muriatic solution, separated 7 parts of oxide of iron; and oxalate of ammonia produced a precipitate equivalent to 3 parts of lime.

This chemical examination shows, that 100 parts of the resinous cement are formed of

Its component parts.

Yellow resin	49
Pure sand	37
Oxide of iron	7
Lime	3
Loss	4
	100

It appears, that necessity has taught the natives of New Holland a practice, which engravers employ every day. It has taught them, to mix a proper quantity of sand with the yellow resin kept some time in fusion, and thus to compose a cement capable of acquiring considerable hardness.

Similar to engraver's wax,

This is the mode in which the resinous cement, called in the shops engravers' wax, is prepared. Brickdust is added to common resin: the mixture is melted, and cast in moulds: and thus it is formed into red cakes, which are sold to the engravers. I have satisfied myself, that, the oftener this mixture has been melted, the harder it is.

I examined engravers' wax in comparison with the cement of the savages of New Holland; and I observed with surprise, that the proportions of resin and brickdust were precisely the same with those of the yellow resin and sand in the cement I analysed.

but harder.

It appeared to me, however, that the engravers' wax, though very hard, particularly when it has been melted several times, is inferior in solidity to the cement of the natives of New Holland; a difference that may be ascribed to the difference between the resins, and the greater or less force, with which their particles cohere.

XV.

Note on the Precipitation of Silver by Copper: by Mr. GAY-LUSSAC*.

Silver precipitated from its solution by copper impure:

but may be obtained otherwise.

MOST chemists are of opinion, that the precipitate, obtained by leaving a slip of copper in a solution of nitrate of silver, is an alloy of the two metals, and that consequently it is impossible to procure pure silver by this process. This is the truth of the fact, when no attention is paid to particular circumstances: but if we examine the different stages of the precipitation, and attend to the causes that produce them, we shall soon perceive, that it is easy to obtain silver free from the copper by which it is precipitated.

The first portions separated pure:

and the whole may be rendered so by adding nitrate of silver.

In fact, the first portions of silver separated are commonly pure, and do not give a blue tinge to ammonia, when they are dissolved in nitric acid. It is only in proportion as the copper enters into solution, that we find any in the precipitate; so that toward the end of the process the quantity becomes very evident. If therefore we separate the first portions of silver, we shall find them exempt from copper: but, to obtain considerable quantities, we may take the whole of the precipitated silver, as I have done, wash it, and digest it with a small quantity of nitrate of silver: by these means, the copper will be redissolved, and a corresponding quantity of silver precipitated.

An affinity between the metals may occasion an alloy to fall down, but not in this case.

Galvanism acts in the precipitation.

I am far from thinking, that the mutual action of metals is incapable of occasioning the formation of alloys in metallic precipitations: I only conclude, that, in the experiment I have just related, the precipitation of the copper is not occasioned by the affinity between this metal and silver; since in this case we ought to have the same alloy in every stage of the precipitation, and besides this could not be destroyed by being placed in contact with a fresh quantity of nitrate of silver. Precipitation in general being the effect of a galvanic process; it appears to me, that the copper, which is reduced by hidrogen as well as silver, is precipitated with this metal by the same cause. Many other metallic precipitations would exhibit similar results.

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* Annales de Chim. vol. LXXVIII, p. 91.

XVI.

Table expressing the Quantities of Sulphuric Acid at 66° [spec. grav. 1·842] contained in Mixtures of this Acid and Water at different Degrees of the Areometer; by Mr. VAUQUELIN*.

Strength of sulphuric acid an object of inquiry to manufacturers.

THE use that is made at present of sulphuric acid of different strengths for various uses, and particularly for the manufacture of soda, has rendered it necessary for the manufacturers and consumers of this acid, to inquire into the quantity of concentrated acid, that is, at 66°, indicated by the different degrees of the areometer.

Best strength for decomposing sulphuric acid.

Concentrated sulphuric acid not being necessary for the decomposition of muriate of soda, that which is carried to 50° in the chamber being even preferable, both the manufacturer and consumer would find their advantage in the use of this. But to settle the price of this acid, according to the various degrees marked by the areometer, we must know how much acid of 66° there is at each degree, which can be found only by experiment; the quantities of acid not being in the direct ratio of the degrees, in consequence of the condensation that takes place on the combination of the acid with water.

A table of strengths useful.

Having been very frequently consulted on this subject, I have thought it would be useful, to construct a table by means of experiments, in which the degrees of the areometer should show the weights of acid at 66°.

Method in which the following was constructed.

For this purpose I began with taking accurately the specific gravity of the sulphuric acid at 66°, which I used in making my mixtures; and I found it to be 1·842, distilled water being taken as the unit, at the temperature of 12° of Reaum. [59° F.]. I then sought the quantities of this acid and water necessary to produce the degrees of the areometer used in trade to measure the density of this acid, beginning at 60°, and proceeding downward by fives till I came to 5°.

The weights were ascertained with great care by means of a very sensible balance; the vessel, in which I made my mixtures, was constructed so that the vapours formed by the heat evolved in each instance could not escape; and I was careful not to take the degree on the areometer, till the mixture had returned to 12° R. [59° F.].

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* Annal. de Chim vol. LXXVI, p 260.

I reduced to hundredth parts the quantities of water required to obtain the degrees on the areometer, which necessarily gave me fractions.

It may be objected, that the intervals in my table are too great; and I confess it would have been better, to make as many mixtures as the concentrated acid marks degrees on the areometer, namely 66: but, not to mention that this would have rendered my undertaking tedious and difficult, it would not have been of any great use for the purposes of trade, for which it was chiefly intended. In fact, the quantity of acid in any degree in these intervals may be obtained very nearly by means of a simple sum in the rule of proportion.

Lastly, I have taken the specific gravity of each of my mixtures, which will give the means of ascertaining the quantity of acid and water in such mixtures, when an areometer is not at hand. These specific gravities too will show the degree of condensation, that water experiences, in combining with sulphuric acid in the different proportions employed.

Table of mixtures of sulphuric acid and water.

Deg. of Areom.	Specif. gravity.	Sulph acid at 66°.	Water.
5	1·023	6·60	93·40
10	1·076	11·73	88·27
15	1·114	17·39	82·61
20	1·162	24·01	75·99
25	1·210	30·12	69·88
80	1·260	36·52	63·48
35	1·315	43·21	56·79
40	1·375	50·41	49·59
45	1·466	58·02	41·98
50	1·524	66·45	33·55
55	1·618	74·32	25·68
60	1·725	84·22	15·78
66	1·842		

A

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SUPPLEMENT TO VOL. XXX.

ARTICLE I.

On the Place of a Sound, produced by a musical String. In a Letter from Mr. JOHN GOUGH.

To. MR. NICHOLSON.

SIR,

Form- vations to.

CERTAIN experiments and remarks of mine on the augmentation of sounds appeared in the tenth volume of your Journal; the intention of which communication was to show, that the range of a sound may be greatly extended, by enlarging the vibrating surface, while the magnitude of the impulse remains the same. Among other remarks contained in that paper, a fact is mentioned; which proves, that the audible effect of a musical string varies with the texture of the instrument to which it is attached; or, to use the language of certain writers on acoustics, the force of such a string depends not a little on the conducting power of the frame upon which it is stretched.

Remarks on the common theory of musical strings.

Perhaps this assertion will be called a novelty in the theory of stringed instruments; for I believe, that the philosophers, who have turned their thoughts to the subject, are unanimous in maintaining, that the effect, which a vibrating fibre produces on the ear, proceeds solely from the pulses, excited in the air by the undulatory motion of the cord. In consequence of this doctrine, they make a musical string to be the seat of the sound which it occasions, in the same manner as a bell, a drum, and a tambarine may be called the seats of the sounds, which they impart to the ear through the medium of the atmosphere. Though I do not deny, that pulses are produced in the air by slender fibres in the act of vibration, I have long disputed the accuracy of the prevailing theory, without being able to demonstrate the truth of the suspicion to my own satisfaction. An accidental observation, however, attracted my notice lately, which proves the string to be the exciting cause; and shows, that the sound proceeds from the frame or body of the instrument, in the same manner that the sound of a bell proceeds from that vessel. The circumstance here alluded to suggested the following easy experiments; which any one may repeat, who wishes to be convinced of the fact by his own experience.

Two experiments contradicting the received theory.

Exp. 1. One end of an iron wire (No. 28) was fastened to a brass knob screwed into a table of deal, and the other end was wrapped round a slender cylinder of yew; four or five inches long. The wire measured six feet betwixt the knob and cylinder, and I stretched it with considerable force, by holding the wooden pin in my hand, so as to let no part of the string touch my fingers. The wire being then made to vibrate, the sound, produced on the occasion, came from the table; not only in my opinion, but also in the judgment of several persons, before whom the experiment has been repeated at different times.

Exp. 2. If, in stretching the wire, one end of the yew cylinder was made to press upon a second table, placed five feet from that into which the brass knob was fixed, the surface became the seat of sound, that supported my hand and the wooden pin. But when the cylinder was removed to a small distance from the table, on which it pressed, and the wire was kept stretched at the same time, the sound was heard instantaneously as in the first experiment proceeding from the opposite table. It seems adviseable to remind the reader of a well known fact, before the inferences are stated, which appear to be deducible from the preceding experiments. When a number of sounds strike the ear at the same time, one of which is much more powerful than any of the rest; all the weaker escape notice, and the seat of the strongest is alone recognised: in more familiar language

A well known fact stated.

guage it is the only one of the number, which the observer hears.

A vibrating string performs the part of a drumstick on a harpsichord, violin, &c

Now three sets of vibrations are evidently going on at the same time, in the first experiment; these are the primary set of the wire, and the two derivative sets existing in the table at one end of it, and in the wooden pin at the other. But the length of the string enables the ear to ascertain which one of the three sets gives the seat of the sound; and this is the vibratory motion of the table; consequently the table is the sounding body, and the wire does nothing more than perform the part of a drumstick in causing the surface of it to vibrate with great celerity. This discovery points out a distant analogy connecting the thundering noise of a drum and the smooth sounds of a harp or lute. They are, however, very distinct to sense for obvious reasons: the cover of the former instrument is highly elastic, and the sound of it continues to die away for some seconds after it has been struck; each stroke of the drumstick renews this sound, and the interval between two succeeding strokes is sufficiently large to be observed by the ear; and hence proceeds the thundering noise of a drum. On the contrary, the sound derived from the wooden frame of a stringed instrument by a single stroke is very transient; but the impulses of the strings beat upon it with a celerity, which does not permit the sound to suffer a sensible diminution of force in the interval of two successive strokes; which is the cause of smoothness in tones of this sort.

The necessity of extensive surfaces in the production of sound.

In the second experiment, the vibrations, communicated from the wire to the cylinder of yew, are imparted by contact to the other table, which thereby becomes the seat of sound; because, being nearer the person of the experimenter, it makes a more powerful impression on his ear, than the first table; which stands at a greater distance from him. But so soon as the cylinder ceases to touch the board, that supports it, the experimenter hears the sound from the opposite table; notwithstanding it, is farther from his ear than the wooden pin in his hand. Hence we discover the utility, and even the necessity of extensive surfaces in the production of sounds; for the impulses received from the vibrating wire, by the table and cylinder, are equal in number and magnitude; but of the three sets of contemporary vibrations, that existing in the table is alone heard.

The frame of a stringed instrument is the seat of sound.

The foregoing facts and observations demonstrate, that the pulses excited in the air by a vibrating cord do not make any sensible impression on the organs of hearing; on the contrary, the sound, which we attribute to a musical string, comes in reality from the frame, upon which it is stretched. This errour of judgment arises from the proximity of the cord and frame, which prevents the ear from determining whether of the two is the sonorous body; we therefore ascribe the sound to the part that sustains the impulse. It is true, indeed, that the notes of a harpsichord or violin are caused by the vibrations of the strings; but then the various modifications, incident to these rapid and delicate motions, are imparted to the ear through the medium of the less elastic frame; the momentary sounds of which change their character when acted upon by a quick succession of impulses, and become continuous.

Middleshaw, *Dec*. 6, 1811. JOHN GOUGH.

II.

Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons; By B. C. Brodie, *Esq. F. R. S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.*

(Concluded from p. 307.*)*

Poisons applied to wounds.

III. *Experiments with Poisons applied to wounded Surfaces.*

Experiments with the essential Oil of Almonds.

Essential oil of almonds. Experiment 16.

Exp. 16. I MADE an incision in the thigh of a rabbit, and introduced two drops of essential oil between the skin and the muscles. In four minutes after the application, he was seized with violent convulsions, and became insensible, and in two minutes more he was apparently dead; but the heart was felt through the ribs acting one hundred and twenty times in a minute, and it continued acting for several minutes. There were no other appearances in the limb, than would have resulted from an ordinary wound.

Experiment 17.

Exp. 17. Two drops of the essential oil of almonds were introduced into a wound in the side of a mouse. Two minutes afterward he was affected with symptoms similar to those which occurred in the last experiment, and in two minutes more he was apparently dead; but the heart continued to contract for some minutes afterward.

Acts as when applied to the tongue, but less quickly.

From the experiments which I have just related, and from others which it appears unnecessary to detail, as the general results were the same, I have learned, that, where the essential oil of almonds is applied to a wound, its effects are not so instantaneous as when it is applied to the tongue; otherwise there is no difference in its effects, in whatever manner it is applied.

Experiment with the Juice of the Leaves of Aconite.

Juice of aconite Experiment 18

Exp. 18. I made a wound in the side of a young rabbit, and introduced between the skin and muscles about twenty drops of the juice of aconite. Twenty-three minutes afterward he was affected with symptoms in all essential respects similar to those, which occurred in an experiment already related, where the juice was injected into the rectum; and at the end of forty-seven minutes from the application of the poison, he was apparently dead. Two minutes after apparent death, the heart was found contracting, but very feebly.

Experiments with the Woorara.*

Woorara Experiment 19.

Exp. 19. A small quantity of the woorara in powder was applied to a wound in the side of a guinea pig. Ten minutes afterward the animal was unable to walk; then he became quite motionless, except some slight occasional convulsions. He gradually became insensible, the respirations were laboured, and at the end of fourteen minutes from the application of the poison, the respiration had entirely ceased, and he was apparently dead; but on opening the thorax, the heart was found acting seventy times in a minute,

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* The woorara is a poison, with which the Indians of Guiana arm the points of their arrows. It appears not to differ essentially from the ticunas, which was employed in the experiments of the Abbe Fontana. I am indebted to Dr. E. N. Bancroft, who not only furnished me with some of the woorara, which he had in his possession, but also lent me his assistance in the experiments, which were made with it.

nute, circulating dark coloured blood, and it continued to contract for several minutes afterward. On dissection no preternatural appearances were observed in the brain; nor was there any other appearance in the limb, than would have arisen from an ordinary wound.

Experiment 20.

Exp. 20. I made a wound in the side of a guinea pig, and introduced into it about two grains of the woorara in powder. At the end of twenty-five minutes, symptoms took place very similar to those, which occurred in the last experiment, and in thirteen minutes more the animal was apparently dead; but the heart continued to contract one hundred and eight times in a minute, and by means of artficial respiration the circulation was kept up for more than twenty minutes.

It acts on the brain.

The results of other experiments, which I have made with the woorara, were similar to those just described. The heart continued to act after apparent death, and the circulation might be kept up by means of artificial respiration. It is evident, that this poison acts in some way or other on the brain, and that the cessation of the functions of this organ is the immediate cause of death.

Best applied dissolved in water.

I found in these experiments, that the best mode of applying the woorara is when it is dissolved in water to the consistence of a thin paste. I first made the wound, and then smeared the poison over it with the end of the scalpel. I found that the animal was more speedily and certainly affected, if there was some hæmorrhage; unless the hæmorrhage was very copious, when it produced an opposite effect, by washing the poison away from the wound. When the poison was applied in large quantity, it sometimes began to act in six or seven minutes. Never more than half an hour elapsed from the time of the poison being inserted, to that of the animal being affected, except in one instance, where a ligature was applied on the limb, which will be mentioned afterward. The woorara, which I employed, had been preserved for some years, which will account for its having been less active, than it has been described to be by those, who had witnessed its effects when in a recent state.

Probably weak from age.

Experiments with the Upas Antiar *.

Upas antiar. Experiment 21.

Exp. 21. About two grains of this poison were made into a thin paste with water, and inserted into a wound in the thigh of a dog. Twelve minutes afterward he became languid; at the end of fifteen minutes, the heart was found to beat very irregularly, and with frequent intermissions; after this, he had a slight rigour. At the end of twenty minutes, the heart beat very feebly and irregularly; he was languid; was sick and vomited; but the respirations were as frequent and as full as under natural circumstances, and he was perfectly sensible. At the end of twenty minutes, he suddenly fell on one side, and was apparently dead. I immediately opened into the thorax, and found the heart distended with blood in a very remarkable degree, and to have entirely ceased contracting. There was one distinct and full inspiration, after I had begun making the incision into the thorax. The cavities of the left side of the heart contained scarlet blood, and those of the right side contained dark coloured blood, as in a living animal.

Experiment 22.

Exp. 22. A small quantity of the upas antiar, prepared as before, was inserted into a wound in the thigh of a young cat. She appeared languid in two minutes after the poison was inserted. The symptoms, which took place, did not essentially differ from those, which occurred in the last experiments, except that there were some convulsive motions of the limbs. At eight minutes after the poison was inserted, she lay on one side, motionless and insensible; the heart could not be felt, but the respiration had not entirely ceased. On opening into the thorax, I found the heart to have ceased contracting. It was much distended with blood: and the blood in the cavities of the left side was of a scarlet colour. There were two full inspirations after the incision of the thorax was begun. On irritating the heart with the point of the scalpel, slight contractions took place in the fibres of the appendices of the auricles, but none in any other part.

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* We are informed, that the island of Java produces two powerful vegetable poisons, to one of which the natives give the name of *upas tieutè*, and to the other that of *upas antiar*. I was supplied with a quantity of the latter through the kindness of Mr. Marsden, who had some of it in his possession.

Experiment 23.

Exp. 23. The experiment was repeated on a rabbit. The symptoms produced were similar to those in the last experiments; but the animal did not vomit, and the convulsive motions were in a less degree: he died eleven minutes after the poison was inserted. On opening the chest, the heart was found to have entirely ceased contracting; it was much distended with blood; and the blood in the cavities of the left side was of a scarlet colour. On irritating the heart with the point of the scalpel, the ventricles contracted, but not sufficiently to restore the circulation.

Experiment 24

Exp. 24. About a grain of the upas antiar was inserted into a wound in the side of a rabbit. He was affected with symptoms similar to those before described, and died in ten minutes after the poison was applied. On opening the thorax immediately after death, the heart was found to have ceased contracting, and the blood in the cavities of the left side was of a scarlet colour.

It appears to act like the infusion of tobacco, on the heart.

It appears from these experiments, that the upas antiar, when inserted into a wound, produces death (as infusion of tobacco does when injected into the intestine) by rendering the heart insensible to the stimulus of the blood, and stopping the circulation. The heart beats feebly and irregularly, before either the functions of the mind, or the respiration appear to be affected. Respiration is performed even after the circulation has ceased; and the left side of the heart is found after death to contain scarlet blood, which never can be the case, where the cause of death is the cessation of the functions of the brain or lungs. The convulsions, which occur when the circulation has nearly ceased, probably arise from the diminution of the supply of blood to the brain, resembling those, which take place in a person, who is dying from hæmorrhage.

How do poisons applied to wounds act on the brain?

There remains an interesting subject of inquiry, "through what medium do poisons influence the brain, when applied to wounds?" That poisons applied in this manner do not produce their effects precisely in the same way as poisons taken internally, is rendered probable by this circumstance;

that some poisons, which are very powerful when applied to wounds even in small quantities, are either altogether inefficient when taken internally, or require to be given in very large quantities, in order to produce their effect; and *vice versâ*.

Three possible ways.

A poison applied to a wounded surface may be supposed to act on the brain in one of three ways,

1. By means of the nerves, like poisons taken internally.
2. By passing into the circulation through the absorbent vessels.
3. By passing directly into the circulation through the divided veins.

Experiment 25. The nerves divided,

Exp. 25. In order to ascertain, whether the woorara acts through the medium of the nerves, I exposed the axilla of a rabbit, and divided the spinal nerves supplying the upper extremity, just before they unite to form the axillary plexus. The operation was performed with the greatest care. I not only divided every nervous filament, however small, which I could detect, but every portion of cellular membrane in the axilla, so that the artery and vein were left entirely insulated. I then made two wounds in the fore arm, and inserted into them some of the woorara formed into a paste. Fourteen minutes after the poison was applied, the hind legs became paralytic, and in ten minutes more he died, with symptoms precisely similar to those, which took place in the former experiments, and the heart continued to act after apparent death. On dissection, the nerves of the upper extremity were particularly examined, but not the smallest filament could be found undivided.

the effects the same.

I made the following experiment to ascertain whether the woorara passes into the circulation through the absorbent vessels.

Experiment 26. The thoracic duct tied,

Exp 20. I tied a ligature round the thoracic duct of a dog, just before it perforates the angle of the left subclavian and jugular veins. I then made two wounds in the left hind leg, and introduced some of the woorara in powder into them. In less than a quarter of an hour he became affected with the usual symptoms, and died in a few minutes afterward.

the effects the same.

After death, I dissected the thoracic duct with great care. I found it to have been perfectly secured by the ligature. It was very much distended with chyle, and about two inches below its termination its coats had given way, and chyle was extravasated into the cellular membrane. The lymphatic vessels in the left axilla were distended in a very remarkable degree, and on dividing them, not less than a drachm of lymph issued from the divided ends.

The poison probably therefore passes through the veins.

Since neither the division of the nerves, nor the obstruction of the thoracic duct interfere in the slightest degree with the effects of the woorara, there is presumptive evidence, that it acts on the brain by entering the circulation through the divided veins. I endeavoured to ascertain, by experiment, whether this is really the case.

To apply ligatures to the large vessels of a limb only would evidently lead to no satisfactory conclusion, since the anastomosing vessels might still carry on the circulation. The only way, which I could devise, of performing the experiment, was to include all the vessels, small as well as large, in a ligature.

Experiment 27. The blood vessels included in a ligature,

Exp. 27. In order to make the experiment the more satisfactorily, I exposed the sciatic nerve of a rabbit in the upper and posterior part of the thigh, and passed under it a tape half an inch wide. I then made a wound in the leg, and having introduced into it some of the woorara mixed with water, I tied the tape moderately tight on the fore part of the thigh. Thus I interrupted the communication between the wound and the other parts of the body by means of the vessels, while that by means of the nerve still remained. After the ligature was tightened, I applied the woorara a second time, in another part of the leg. The rabbit was not at all affected, and at the end of an hour I removed the ligature. Being engaged in some other pursuit, I did not watch the animal so closely as I should otherwise have done; but twenty minutes after the ligature was removed, I found him lying on one side, motionless and insensible, evidently under the influence of the poison; but the symptoms were less violent than in most instances, and after lying in this state he recovered, and the limb became perfectly warm, and he regained the power of using it.

the animal not affected, till the ligature was removed.

Experiment 28.

Exp. 28. I repeated the last experiment with this difference, that after having applied the poison, I made the ligature as tight as I could draw it. I removed the ligature at the end of an hour and twenty minutes, but the animal was not at all affected either before or after the removal of the ligature, and on the following day he had recovered the use of the limb.

Experiment 29.

Exp. 29. I repeated the experiment a third time, drawing the ligature very tight. At the end of forty-five minutes, the animal continued perfectly well, and the ligature was removed. I watched him for three quarters of an hour afterward, but there were no symptoms of his being affected by the poison. On the following day the rabbit died, but this I attribute to the injury done to the limb and sciatic nerve by the ligature, as there was the appearance of inflammation in the parts in the neighbourhood of the ligature.

All confirm the opinion, that the poison enters the veins

These three experiments were made with the greatest care. From the mode, in which the poison was applied, from the quantity employed, and from my prior experience, I should have entertained not the smallest doubt of the poison taking effect in every instance in less than twenty minutes, if no ligature had been applied. In two of the three, the quantity of woorara was more than had been used in any former experiments.

Abbé Fontana's experiments support the same conclusion,

I have not judged it necessary to make any more experiments, with the ligature on the limb; because the numerous experiments of the Abbé Fontana on the ticunas coincide in their results with those, which have just been detailed, and fully establish the efficacy of the ligature, in preventing the action of the poison. It is not to be wondered at, that the ligature should sometimes fail in its effects; since these must evidently depend on the degree, in which the circulation is obstructed, and on the length of time during which the obstruction is continued.

even where they appear unfavourable to it

There can be little doubt, that the woorara affects the brain, by passing into the circulation through the divided vessels. It is probable, that it does not produce its effects, until it enters the substance of the brain, along with the blood, in which it is dissolved; nor will the experiments of the Abbé Fontana, in which he found the ticunas produce almost instant death, when injected into the jugular vein of a rabbit, be found to militate against this conclusion; when we consider how short is the distance, which, in so small an animal, the blood has to pass from the jugular vein to the carotid artery, and the great rapidity of the circulation; since in a rabbit under the influence of terrour, during such an experiment, the heart cannot be supposed to act so seldom as three times in a second.

I have made no experiments to ascertain through what medium other poisons, when applied to wounds, affect the vital organs, but from analogy we may suppose, that they enter the circulation through the divided blood-vessels.

IV.

Death from destroying the functions of the brain.

The facts already related led me to conclude, that alcohol, the essential oil of almonds, the juice of aconite, the oil of tobacco, and the woorara, occasion death simply by destroying the functions of the brain. The following experiment appears fully to establish the truth of this conclusion.

Experiment 30.

Exp. 30. The temperature of the room being 58° of Fahrenheit's thermometer, I made two wounds in the side of a rabbit, and applied to them some of the woorara in the form of paste. In seven minutes after the application, the hind legs were paralysed, and in fifteen minutes respiration had ceased, and he was apparently dead. Two minutes afterward the heart was still beating, and a tube was introduced through an opening into the trachea, by means of which the lungs were inflated. The artificial respiration was made regularly about thirty-six times in a minute.

At first, the heart contracted one hundred times in a minute.

At the end of forty minutes, the pulse had risen to one hundred and twenty in a minute.

At the end of an hour, it had risen to one hundred and forty in a minute.

At the end of an hour and twenty-three minutes, the pulse had fallen to a hundred, and the artificial respiration was discontinued.

At the commencement of the experiment, the ball of a thermometer being placed in the rectum, the quicksilver rose to one hundred degrees; at the close of the experiment it had fallen to eighty-eight and a half.

During the continuance of the artificial respiration, the blood in the femoral artery was of a florid red, and that in the femoral vein of a dark colour, as usual.

Immediate cause of sudden death.

It has been observed by Mr. Bichat, that the immediate cause of death, when it takes place suddenly, must be the cessation of the functions of the heart, the brain, or the lungs. This observation may be extended to death under all circumstances. The stomach, the liver, the kidneys, and many other organs, are necessary to life, but their constant action is not necessary; and the cessation of their functions cannot therefore be the immediate cause of death. As in this case the action of the heart had never ceased; as the circulation of the blood was kept up by artificial respiration for more than an hour and twenty minutes after the poison had produced its full effects; and as during this time the usual changes in the colour of the blood took place in the lungs; it is evident, that the functions of the heart and lungs were unimpaired: but that those of the brain had ceased, is proved, by the animal having continued in a state of complete insensibility; and by this circumstance, that animal heat, to the generation of which I have formerly shown the influence of the brain to be necessary, was not generated.

Perhaps life might be preserved by keeping up artificial respiration.

Having learned, that the circulation might be kept up by artificial respiration for a considerable time after the woorara had produced its full effects, it occurred to me, that, in an animal under the influence of this or of any other poison, that acts in a similar manner, by continuing the artificial respiration for a sufficient length of time after natural respiration had ceased, the brain might recover from the impression, which the poison had produced, and the animal might be restored to life. In the last experiment, the animal gave no sign of returning sensibility: but it is to be observed, 1. That the quantity of the poison employed was very large. 2. That there was a great loss of animal heat, in consequence of the temperature of the room being much below the natural temperature of the animal, which could not therefore be considered under such favourable circumstances as to recovery, as if it had been kept in a higher temperature. 3. That the circulation was still vigorous when I left off inflating the lungs; and therefore it cannot be known what would have been the result, if the artificial respiration had been longer continued.

Experiment 30. Confirms this opinion.

Exp. 30. A wound was made in the side of a rabbit, and one drop of the essential oil of almonds was inserted into it, and immediately the animal was placed in a temperature of 90°. In two minutes he was under the influence of the poison. The usual symptoms took place, and in three minutes more respiration had ceased, and he lay apparently dead, but the heart was still felt beating through the ribs. A tube was then introduced into one of the nostrils, and the lungs were inflated about thirty-five times in a minute. Six minutes after the commencement of artificial respiration, he moved his head and legs, and made an effort to breathe. He then was seized with convulsions, and again lay motionless, but continued to make occasional efforts to breathe. Sixteen minutes after its commencement, the artificial respiration was discontinued. He now breathed spontaneously seventy times in a minute, and moved his head and extremities. After this, he occasionally rose, and attempted to walk. In the intervals, he continued in a dozing state; but from this he gradually recovered. In less than two hours he appeared perfectly well, and he continued well on the following day.

Inflating the lungs recommended in different instances.

The inflating the lungs has been frequently recommended in cases of suffocation, where the cause of death is the cessation of the functions of the lungs: as far as I know, it has not been before proposed in those cases, in which the cause of death is the cessation of the functions of the brain*. It is probable, that this method of treatment might

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Experiments of Mr. Delile.

* Since this paper was read, I have been favoured by the Right Hon. the President with the perusal of a Dissertation on the Effects of the Upas Tieuté, lately published at Paris by Mr. Delile; by which I find, that he had employed artificial respiration for the recovering animals, which were under the influence of this poison, with success. Mr. Delile describes the upas tieute as causing death by

might be employed with advantage for the recovery of persons labouring under the effects of opium, and many other poisons.

V.

General conclusions.

The experiments, which have been detailed, lead to the following conclusions.

1. Alcohol, the essential oil of almonds, the juice of aconite, the empyreumatic oil of tobacco, and the woorara, act as poisons by simply destroying the functions of the brain; universal death taking place, because respiration is under the influence of the brain, and ceases when its functions are destroyed.

2. The infusion of tobacco, when injected into the intestine, and the upas antiar, when applied to a wound, have the power of rendering the heart insensible to the stimulus of the blood, thus stopping the circulation; in other words, they occasion syncope.

3. There is reason to believe, that the poisons, which in these experiments were applied internally, produce their effects through the medium of the nerves, without being absorbed into the circulation.

4. When the woorara is applied to a wound, it produces its effects on the brain, by entering the circulation through the divided blood-vessels; and, from analogy, we may conclude, that other poisons, when applied to wounds, operate in a similar manner.

5. When an animal is apparently dead from the influence of a poison, which acts by simply destroying the functions of the brain, it may, in some instances, at least, be made to recover, if respiration is artificially produced, and continued for a certain length of time.

From analogy we might draw some conclusions respecting the mode, in which some other vegetable poisons produce their effects on the animal system; but I forbear to enter into any speculative inquiries; as it is my wish, in the present communication, to record such facts only, as appear to be established by actual experiment.

by occasioning repeated and long continued contractions of the muscles of respiration, on which it acts through the medium of the spinal marrow, without destroying the functions of the brain.

III. *Description*

III.

Description of a Machine for Washing Potatoes and other esculent Roots for feeding Cattle: by Mr. William Lester, *of Paddington**.

SIR,

Machine for cleaning roots.

HEREWITH you will receive a machine for the more expeditious washing of all tuberous rooted vegetables (such as potatoes, turnips, carrots, &c.) from the soil that adheres to them when taken from the ground.

Disadvantages of an old one for the purpose.

The staved cylinder, revolving in a trough of water so slow as not to excite the centrifugal force, is not new. I have made use of it myself twelve years ago, but always found it cold and wet work, to take the roots from it when washed. To obviate which, I have added the levers and wheels, and find it a very great improvement, as a boy therewith can do the work of two men, without exposing himself to the dangerous effects of dabbling in cold water. The importance of this mode will appear very obvious, when compared with the present laborious one used by the potato sellers in London. The partial motion given to the potatoes, by stirring them about in a tub, cannot separate the soil so effectually from them, as when the water is more violently agitated by their falling over each other in a revolving cylinder, neither will they be so much bruised as by the ends of the levers. If the soil should be particularly adhesive, the heads of a couple of old heath or birch brooms put into the cylinder will effectually disengage it from the eyes of the potatoes, and as the dirt separates, it falls to the bottom of the water in the vessel under the cylinder.

These removed.

If you will have the goodness to lay this before the Society, and it should be deemed worthy of their attention, I will, if necessary, on being requested, attend to explain the effects of the machine.

I am, Sir,
Your most humble and obedient servant,
W. LESTER.

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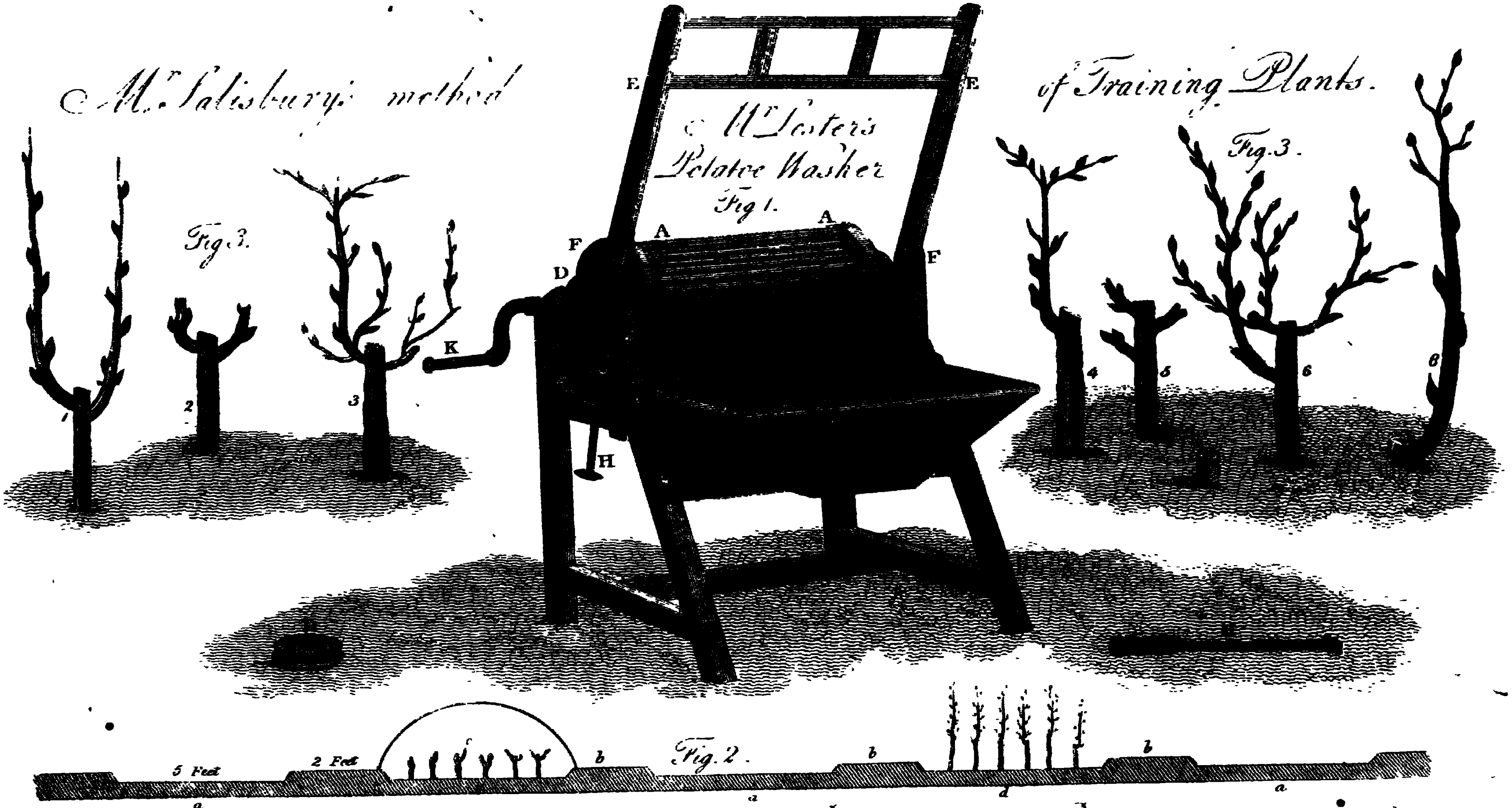
* Trans. of the Society of Arts, vol. xxvii, p. 34. The silver medal was voted to Mr. Lester.

SIR,

Mr. Salisbury's method of Training Plants.



Section of Mr. Salisbury's Beds for young Plants.

SIR,

Great saving of labour in washing potatoes with it

AGREEABLE to your request I have procured the enclosed certificates, &c., on the utility of my improved root washer, which you will have the goodness to lay before the Society.

I have no doubt but it would save half the labour in washing potatoes in London, if it were brought into use: It is obvious to every one who has seen it work, that it is greatly superior to the tub and levers used by the potato-merchants, as it is not so liable to injure the roots. The soil is drawn from them with more facility, and their falling into the basket from the cylinder is more clean and commodious by far than taking them out of the tub with a grated shovel, from the corners of which many roots are bruised: it also prevents the potatoes being injured in quality from being long soaked in water, from which they suffer greatly in the common way.

I am, Sir,
Your most obedient and humble servant,
W. LESTER.

Certificates of the Utility of Mr. LESTER'S *Machine for washing Tuberous Roots.*

SIR,

Testimonies of its utility.

IN reply to your inquiries respecting the utility of the root-washer, which I purchased of you about twelve months since, I have much satisfaction in stating, that I have used it, constantly, during the last winter, and have found it to answer the purpose for which it is intended most thoroughly; and if my opinion will be of any benefit to you, I have not the least objection to your making it public.

I am, Sir,
Your obedient humble servant,
JAMES JOHN FARQUHARSON.

SIR,

IN answer to yours I have to observe, I consider your root-washer to be a machine that no farmer, who is in the habit of giving roots to his stock, ought to be without. I use it constantly in washing potatoes for 150 fattening sheep, beside hogs. A man, and a boy ten years old, will wash, without any exertion, 20 bushels an hour, or a man alone will do half the quantity. I have tried a few parsnips with it, and find it do them equally as well, and have no doubt but any kind of roots may be washed with it. I am very much pleased with it, and so must every one who has tried it.

With every wish for your success,
I am, yours sincerely,
JOHN CLARKE.

To Mr. LESTER, *&c.*

LORD NORTHAMPTON acquaints Mr. Lester, that the potato-washer, that was bought of him, answers the purpose perfectly well, and is approved by all who have used it.

Decription of Mr. LESTER'S *Machine for washing Potatoes, &c. Plate IX, Fig.* 1.

Description of the machine.

THIS machine is shown in plate IX, fig. 1. The potatoes are put into a cylinder or lantern A A, formed of two circular boards, and a number of staves connecting them. Six of these staves are connected at the ends of two pieces of wood, so that they can be opened as a door, to put in or take out the potatoes. The cylinder turns round in a trough B B, filled with water, and supported on four legs. On the end of the axis of the cylinder, two pulleys, one of which is shown separately at D, are loosely fitted; these are intended for the cylinder to move upon, when full of potatoes; they run upon a swinging frame E E, which rests on centres at F F: when the long end of the frame is pulled down, tbe other end is raised up, lifting the cylinder out of the trough B B; when the long end of the frame becomes the lowest, the cylinder rolls down on its wheels D, till it is over the hopper or wooden funnel G, under which a wheelbarrow or basket to receive the clean potatoes is placed: the door of the cylinder is now opened, and the contents turned out through the hopper into the vessel beneath it. When the frame is in this situation, the iron rods H, which are jointed to the short ends of the levers, form stops to the farther descent of the frame.

Mode of using it.

When fresh quantities of potatoes are to be washed, they are thrown in at the door of the cylinder, which is then shut up, and held shut by two small bolts. The end of the frame E is then raised up, so as to make the short end the lowest, and the cylinder runs down on its two wheels D over the trough B, till it is stopped by two iron prongs fixed on the end of the frame E; the cylinder is then suffered to fall down into the trough, and the potatoes, &c. are washed by turning it round by its handle K. *a* is a plug to let out the foul water.

Advantages.

Any person who has seen the laborious and imperfect method of washing potatoes in a tub, as practised in London, will be convinced of the utility of this machine, and of its preserving the potatoes from being water soaked and spoiled, which is the case when they are long immersed in water.

IV.

Method of packing Plants and Trees intended for Exportation, so as to preserve their vegetative Powers for many Months: by WILLIAM SALISBURY, *of the Botanic Gardens at Brompton and Sloane-Street* *.

SIR,

Discovery of a substance that will preserve plants in a growing state several months.

WHEN I had the pleasure of seeing you last Spring, I mentioned a supposed discovery I had made of a substance, that would preserve trees and plants for a considerable time in a growing state, when packed up in close boxes: and that by this method they might be sent abroad to great distances with more success and less trouble than in any other. I now take the liberty of troubling you with the results of several experiments, which I have since made; being certain, that a greater demand will be found for the various articles cultivated in this country, and the persons who are engaged in that trade benefitted, when it is publicly known.

Trees packed in it six months.

A box I have now sent, marked No. 1, contains specimens of tulip trees, and liquid amber trees, which were packed up close from September, 1807, till March, 1808: they were then planted in my nursery; and the whole, amounting to several hundreds, have grown equally as well as they would have done, if only transplanted from one part to another of the same ground.

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* Trans. of the Soc. of Arts, Vol. XXVII, p. 40. Twenty guineas were voted to Mr. Salisbury for this communication.

Several trees sent to America.

In February last I sent to Boston in New England two packages in this way, each containing upwards of nine hundred trees of different kinds; and I have lately received the pleasing intelligence, that they have all arrived safe and done well, but that some fruit trees sent to the same gentleman, packed in the usual way, were all spoiled, owing to the heat of the hold of the vessel, in which all the packages were placed.

Trees packed four months or more.

The other box I now send to you, marked No. 2, contains specimens of different trees, which were packed up by my order, some of which have been in the boxes four months, and others a longer period, and the remainder now in the boxes are all in a similar state of preservation, and I have little doubt will remain three months longer, or more, without injury.

Cause of trees being injured in long journeys.

I must beg leave to observe, that the principal cause why things of this nature do not succeed in long journeys is, that if the package, (as is commonly the case) becomes by any means damp, it is very liable to heat, and the contents to be thereby very much injured: and if left dry, the moisture of the trees becomes exhausted, and they consequently die for want of nourishment. The mode recommended some years ago by Mr. Ellis, of planting the articles in tubs or boxes of earth, is attended with so much trouble, that it has been seldom found to succeed.

Planting in boxes of earth too troublesome.

Properties of the sphagnum palustre, or common bog moss.

In packing my plants, I make use of the long white moss, the *sphagnum palustre* of Linnæus, which grows in great plenty on peat bogs, and, when decayed, forms a great portion of that substance. It differs materially from other vegetables in possessing the power of retaining moisture in a wonderful degree, and it does not appear to be liable to fermentation in any situation, even when laid together in great quantities; hence a decomposition does not readily take place, and it preserves the power of affording moisture and nutriment to plants when completely enveloped in it, as appears by the above experiment.

I am, with great respect, Sir,
Your very obedient, and most humble servant,
WILLIAM SALISBURY.

WE hereby certify that we packed up the several trees and plants at the times marked in the labels of those in the box No. 2, by desire of Mr. Salisbury, and that the said specimens have remained ever since in the boxes as above described.

ALEXANDER REITH.
JOHN WOODHOOD.

DEAR SIR,

Benefits of the study of natural history.

Much still wanting

from the difficulty of keeping seeds

and plants alive.

THE prosperity of a country was never more rapidly promoted, than we have happily witnessed in our own nation within a few years, since the study of natural history has become so general among all ranks of society; and probably nothing has contributed so much thereto as the extended knowledge of botany, and the numerous collections of vegetable productions, which have lately been introduced from all parts of the world. From such sources our agriculture, and many of the arts, have been greatly improved; yet much still remains to be accomplished by the assiduous botanist; for instance, neither the plants producing the cinchona, or which nourish the cochineal, have yet reached our soil, nor are we even acquainted with those which yield many of our most useful drugs. This is owing, in a great measure, to the difficulty of procuring perfect seeds, it being a well known fact, that many kinds will not vegetate, if left dry but a short time after gathering; and the difficulty of keeping plants alive during long voyages has been almost an insuperable obstacle. Impressed, therefore, with the importance of the subject, I wrote to you on the 9th of January last, and have now the pleasure of communicating to the Society of Arts, &c., for the benefit of the public, farther particulars of the mode I have discovered; and by which I am convinced, from actual experiments, trees or plants of all kinds may, with ease and certainty, be transported from any part of the globe to this country and our colonies; being confident, that our commerce will be improved by a more certain mode of exporting the numerous fruits with which our nurseries exclusively abound.

The sphagnum palustre preserves plants in a vegetating state.

I had, some time ago, an opportunity of viewing a large heap of moss (*sphagnum palustre, Linn.*) which had been collected for decorating a grotto. I observed, that, although it had lain exposed for several months in the heat of summer, yet, with the exception of the very outside of the heap, its particles appeared in the same state as when first collected, and that a gentle state of vegetation was still going on. I moreover observed, that several species of heaths, grasses, and plants, that had been by chance collected in the heap, were preserved, and in several instances had the same appearances as when growing; others were a little blanched for want of light; but even these were alive, and capable of growing by proper management. These circumstances led me to make some experiments to ascertain how long trees of different kinds might be preserved in this substance, when excluded from the external air; and I so far succeeded, as to keep them for six months, part of which time had been extreme hot weather, and I had afterward the pleasure of getting them to grow in my garden equal to any, that had been transplanted the same season.

Experiments on it.

Other mosses do not answer the same purpose.

As I have endeavoured to discover what property this particular moss possesses, when compared with others generally used for packing plants, I shall remark, that, as its name implies, it is in a great measure an aquatic, and consequently not liable to injury from moisture, which it has the power of retaining in a wonderful degree, while all the species of hypnum cannot be prevented from rotting, unless they are kept perfectly dry; and although the mosses in general, when moistened with water, are useful to wrap round the roots of trees when packed up, yet they gradually undergo a decomposition; and consequently, if plants were completely enveloped therein, they would decay in time from the same cause, which I have proved in many instances.

I was therefore led to ascribe the advantages, which the *sphagnum palustre* possesses, to its property of holding water, and resisting fermentation; and I am confirmed in this opinion, by a letter, which I have received from my worthy friend Mr. A. T. Thompson, to whom I had submitted some of that moss, for a chemical analysis, and whose letter I now enclose.

Mode of packing plants in it.

The manner in which I have been accustomed to pack up plants is as follows. When the moss is collected from the bogs in which it grows, it should be pressed, in order to drain out as much moisture as possible, and having boxes prepared of sufficient sizes for the young trees, (which may in some instances be shortened in their branches), I lay in the bottom of the box as much moss as will, when pressed with the foot, remain of the thickness of four inches. A layer of the plants should then be put thereon, observing that the shoots of each other do not touch, and that the space of four inches be left round the sides; after this, another layer of moss, about two inches thick, is placed, and then more plants; and I thus proceed, till after the whole of the plants are pressed down as tight as possible, and the box filled within four inches of the top, which space must be filled with the moss: the contents are then trodden down with the foot, and the box nailed closely up.

Treatment of the trees when they arrive.

When trees are intended to be sent to distant countries, I should advise such to be selected as are small and healthy; and when arrived at their place of destination, they should be cut down quite close, even to the second or third eye from the graft, or, in trees not grafted, as near the former year's wood as possible; and having prepared beds according to the following mode, let them be planted therein, to serve as a nursery; for trees of every description suffer so much from removal, that, unless the weather is particularly favourable, they do not recover it for some time, even when only transplanted in their native climate. I do not think it advisable, therefore, to plant them at once, where they are liable to suffer from want of water, and other attentions necessary to their perfect growth. I therefore recommend beds to be thus prepared for them, viz. On some level spot of ground, mark out beds five feet wide, and leave walks or alleys between them, of two feet wide, throwing a portion of the earth out of the beds upon the alleys, so as to leave them four inches higher than the beds.

If the ground is shallow, and the under stratum not fit for the growth of trees, the whole should be removed, and the beds made good with a better soil.

Advantages of this mode.

The advantage arising from planting trees in this way is, that, the beds being lower than the walks, the water which is poured on, for support of the trees, is prevented from running off. The plants are also less exposed to the influence of the winds; and, if a dry and hot season should immediately follow after they are planted, hoops covered with mats, straw, or canvas, may be placed over them, to prevent the sun from burning the plants, and to hinder a too speedy evaporation of moisture.

Shades for the trees.

In warm climates, canvas cloth will answer best for these shades, to be fixed during the heat of the day, so as to prevent the surface of the mould from becoming dry; and if a little water be sprinkled upon the canvas, once or twice during the day, it will keep it tight, and produce a moist atmosphere underneath, which will greatly facilitate the growth of the plants.

These shades should be removed at the setting of the sun, and the plants then watered, when they will also receive the benefit of the dews during the night. In the morning the shades should be replaced, and the plants thus protected till they can stand the open air, to which they should gradually be inured by removing the shades daily more and more, till they can be wholly taken away.

Distance of first planting.

The plants should be planted in rows across the beds, about three inches distance from each other, and the rows should be about nine inches apart; and when the plants have grown thus for one year, they may be removed to the places where they are intended to remain.

I remain, dear Sir, your obedient servant,

WM. SALISBURY.

DEAR SIR,

Analysis of the sphagnum palustre.

THE analysis of the moss, which you put into my hands, has afforded the following result.

A portion of it macerated in boiling distilled water, for twenty-eight hours, yielded a pale straw-coloured, slightly mucilaginous infusion, which was nearly insipid, and of a disagreeable odour.

The infusion of litmus was reddened when added to it. With the nitrate and acetite of barites, insoluble precipitates were thrown down, as was also the case with the acetite of lead. Sulphate of iron gave a very slight olive tinge to the infusion, after standing eight hours; and with the solution of gelatine, a small quantity of a whitish flocculent precipitate was formed, after standing twelve hours. The oxalic acid, a solution of pure ammonia, and the nitrate of silver, produced no effect on the infusion.

The conclusion to be drawn from these results is, that the moss contains in its composition, beside the ordinary principles of vegetables, a very small portion of gallic acid, and of tannin, some sulphuric acid in an uncombined state, mucilage, and extractive matter. No inference can, therefore, be drawn from these results, which explains in any degree the effects of the moss in preserving the vegetables that are enveloped in it; nor is there any effect produced in the air by it, more than is produced by mosses in general, when in an uncorrupted state; other causes to explain the preservative property of the moss must therefore be looked for, and these are to be found, in my opinion, in the peculiar qualities of the moss, connected with its own existence as a living plant.

Plants will not vegetate unless kept alive.

Plants which are taken from the earth, and packed up to be sent abroad, or to any distance so considerable as to keep them for some length of time in the package, will not vegetate when again taken out of it and planted, unless some degree of vitality has been preserved during the period that they have been out of the ground.

Circumstances requisite to this.

To preserve this, four circumstances are essential in the packing material; softness, in order that the delicate parts of the enveloped vegetable be not injured; looseness, that a certain portion of air be contained in it, and that an equal temperature may be preserved; moisture; and the power of resisting fermentation, and the putrefactive process. All of these circumstances this moss possesses in a remarkable degree; its power of absorbing and retaining moisture is more considerable than that which perhaps any other moss possesses, it is light, soft, and loose in its texture, and its vitality is so considerable, as to carry on the powers of

Bog moss eminently possesses these.

of vegetation, and consequently to enable it to resist fermentation and putrefaction for a very great length of time.

Theory of its action,

Placed under such circumstances, the plants, which are packed up in the moss, enjoy a kind of life in some degree similar to that enjoyed by an animal in a torpid state, the functions of life are supported at a very low state, but still sufficient to preserve them in a situation to be acted upon by favourable circumstances, when again planted. Such is the theory I have formed of the effect of this moss in preserving plants; the many necessary calls of my profession have not allowed me time sufficient to investigate the subject, with all the attention I could have wished to have bestowed on it, and must also plead my apology, for the hasty manner in which my opinion is presented to you. I consider the discovery of much value, both to botany and agriculture.

Believe me,
Yours truly,
A. T. THOMPSON.

Dear Sir,

Various fruit trees sent to Sierra Leone, packed in this moss,

IN addition to the account which I delivered to you, respecting my method of packing plants for exportation in the sphagnum palustre moss, I beg leave to observe, that, at the time the case was packed up, which I sent to the Adelphi in January last, a similar package was sent from me to Sierra Leone, by desire of the African Institution, who wished to introduce into that colony the mulberry tree for feeding silk worms; also different kinds of vines, and other fruit trees, amounting in the whole to nearly fifteen hundred trees.

with success.

They arrived there in about four months after the package was made up, and the trees were planted under the direction of a gentleman, to whom I gave a copy of the instructions, which accompanied my former letter to you of last January. The following account of them has since appeared in the African Herald. "A number of fruit and other trees, among which are the white and red mulberry, vines, &c., have been sent from London, by order of the African Institution; all of which are at present growing here, in a very flourishing state; and a piece of ground is clearing in the mountains, to which they are intended to be removed the next season."

African plants sent to England in the same moss,

I requested the gentleman, to whose management the plants were entrusted, to acquaint me how they succeeded, and to use the same moss in packing up for me some of the wild plants of that neighbourhood, which he did in June last; and at the same time I received a letter from Mr. Macaulay of that place, with the following intelligence. "The plants which were bought of you, and sent out by the African Institution, all thrive very well, except the tea tree, sour sop, and a few others. The mulberries, &c., grow most luxuriantly: most of the trees have been removed to a more temperate situation, about three miles hence, where the remainder will soon also be planted."

and arrived in a fit state for vegetating.

This letter arrived by the Derwent, captain Colombine, who also brought me a box of plants packed up in the moss, which had been previously sent with the above; and although the package did not arrive at Brompton before the 5th of October last, the plants were in a fine state of vegetation, and are now growing in my hot-house; and even the moss itself had preserved its vegetative state, and was perfect.

I have been thus particular in my description of the fact, as it is a corroborating proof of the utility of this moss for such purposes; and as the removal of trees cannot be otherwise effected in long voyages, without great expense and inconvenience.

I am, with great respect, Dear Sir,

Yours very truly,

WILLIAM SALISBURY.

Reference to Mr. Salisbury's Method of managing Plants, after they are removed from the Package. See Plate IX, *Fig.* 2, 3.

Method of managing the plants after their arrival.

The plan fig. 2, at the bottom of plate IX, represents, on a small scale, sections of the beds and alleys, with the plants as first set. The beds, *a a*, are to be made on level ground, each bed to be five feet wide, with a space, *b b b*, between each for a road. A portion of the earth is to be thrown out of the five feet beds, upon these roads, so as to raise them four inches higher than the beds, as shown in the plan; *C* represents the plants as first set out, with an arched cover of canvas cloth over them; *D* shows the plants when they have grown in the beds for some time, and in a state ready for planting out.

Directions for pruning them.

To illustrate the mode of cutting or pruning the plants, after they are removed from the package: fig. 3, No. 1, is supposed to be a fruit tree, of one year's growth from the graft, that is a maiden plant. It is to be cut down as is represented in No. 2, and the next season's growth will form the tree No. 3. When it is fit to remain as a dwarf, or if pruned, as is represented in No. 4, it will form a standard, or such as are usually planted in orchards with high stems, in order to be out of the reach of cattle. No 5 is supposed to represent any small tree, that has not been grafted, but cut down for planting. No. 6 is the form it will make the following season, when it may be left; or, should it be intended for timber, or have a crooked stem, cut it close down to the ground as at No. 7, and it will throw up several shoots, which should be all cut off but the strongest, and that will make the tree No. 8. This may afterward be kept trimmed up to a single stem, and a tree be formed much better than in any other mode.

Plants kept alive in the moss nine months.

N. B. The packages of plants, Nos. 1 and 2, mentioned in Mr. Salisbury's first letter, were opened and examined by the committee of agriculture, on the 16th of January, 1809, when all the plants appeared to be in a state fit for vegetation. The boxes were then closed, and placed in the society's model room, and opened again on the 30th of May, at the distribution of the rewards of the society; the plants were then in a state fit for growth, having formed both new roots and branches during their confinement. It appears, therefore, that the plants were, from their first enclosure by Mr. Salisbury, thus preserved nine months out of the earth.

The Sheffield Apparatus for Cleaning Chimneys.

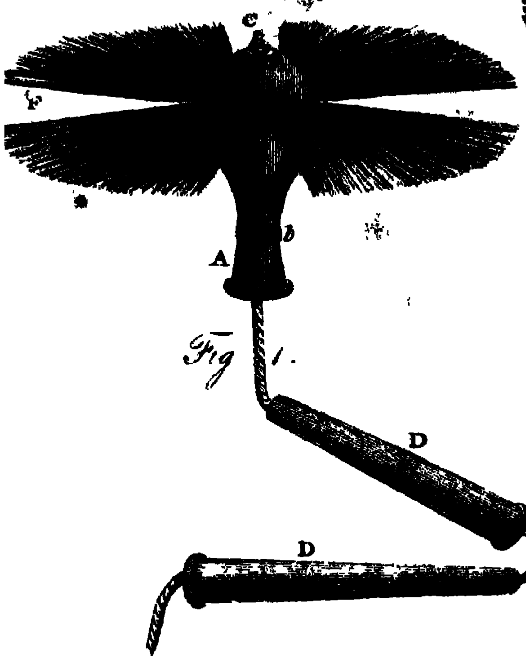

Fig 1.

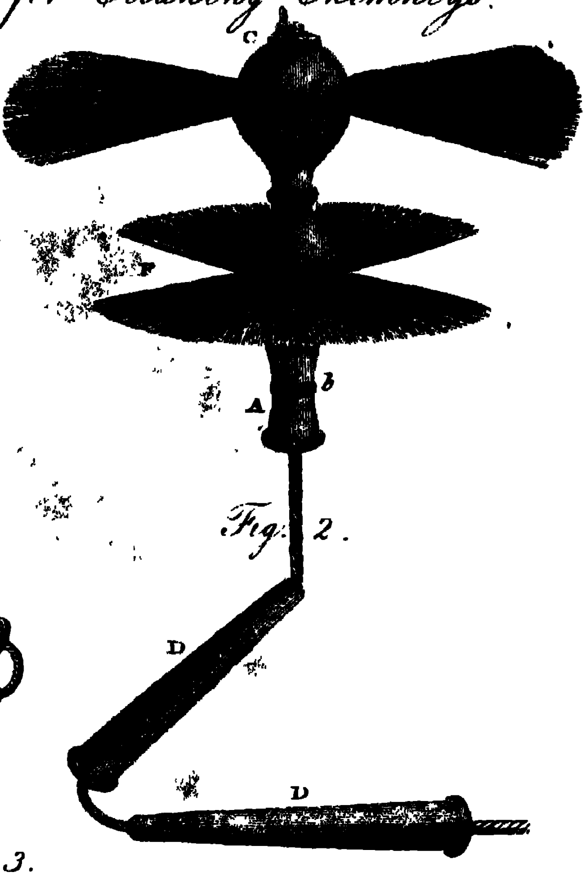

Fig. 2.

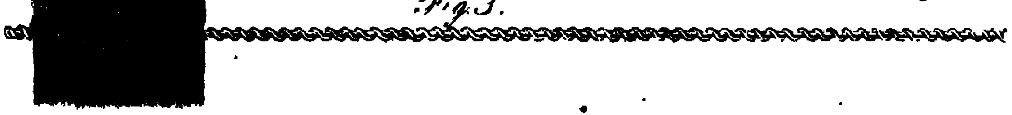

Fig 3.

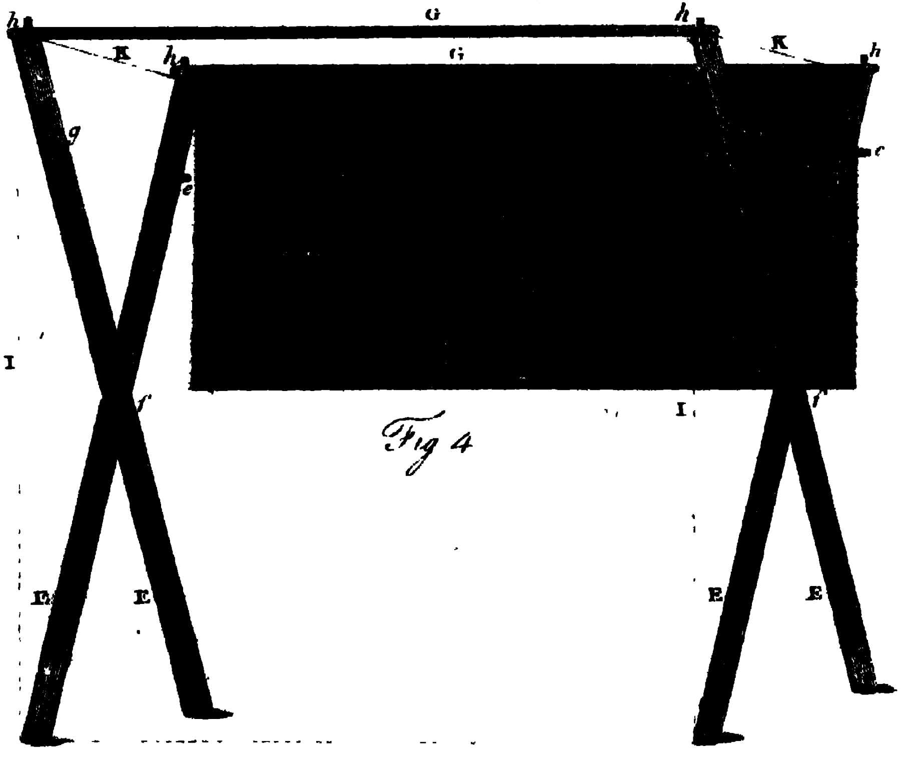

Fig 4

V.

Description of an Apparatus used at Sheffield for cleaning Chimneys: by Mr. Samuel Roberts, *Chairman of a Committee appointed at that Place for encouraging the Sweeping of Chimneys without the use of Climbing-boys**.

Apparatus for cleaning chimneys.

THE two brushes, Plate X, fig. 1, and 2, are those which at present appear to answer best the intended purpose. Fig. 1 is the easiest to work in difficult chimneys; but in those which are tolerably straight No. 2 will be found the more convenient, as it clears itself better of the soot in ascending. Soldered on the inside of the iron hoop A, at *b* is a hollow iron tube, going through the wooden balls B. The nut C screws upon the upper end of the hollow tube, through which the rope passes, and fastens the whole together. The balls B are put upon the tube in separate parts, divided at *d*, for the conveniency of putting in and replacing the brush part F, which is composed of bristles, whisk, and whalebone. The whisk (which should be well selected for the purpose) is in the middle, on each side of which, above and below, is a row of whalebone, split thin, with the flat sides towards the whisk, and above and below the whalebone are bristles. Care must be taken that the whole is not too thick and strong, otherwise it will be difficult to get in and out of the pipes on the tops of the chimneys; where they are pressed together between the balls B, they should not be thicker than three eighths of an inch. Great care must also be taken, that the parts of the brushes are well fastened together, and firmly fixed between the balls B, so as not to be loosened in working. The diameter of the balls B is three inches. The distance between the two brushes FF, in drawing fig. 2, is about four inches. The wooden tubes D, (which are about one inch in diameter,) through which the rope passes, should not be too long; the shortest next the brush should not exceed fifteen inches. They should gradually increase in length as they recede from the brush to the bottom, where they should not exceed thirty inches. The brush, fig. 3, is a good deal similar to a bottle brush, the handle about four feet long, made of whalebone, wrapped with iron or brass wire, the brush part made of bristles only. It will be found to be very useful in cleaning short flues, &c. in kitchen chimneys.

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* Abridged from the Trans. of the Soc. of Arts, vol. xxvii, p. 209. The Society, anxious to relieve the sufferings of humanity, have attended with much pleasure to the endeavours of the inhabitants of Sheffield, and cooperate with them in their attempts to supersede the necessity of employing climbing boys; they have, therefore, immediately on receiving the following communication, ordered it to be inserted in their volume, and an explanatory engraving of the machinery employed to be annexed.

Contrivance for preventing the soot from flying into the room.

Fig. 4 is a kind of tent, within which the machine may be worked. It will be found useful in rooms, where it is particularly desirable, to prevent the least particle of soot from escaping. The cross bars E are of oak, about one inch and a half broad, and half an inch thick, turning upon an iron pin at *f*. G G are two small iron rods, slipping upon pegs at *h*, to each of which is suspended a linen curtain, the one next the chimney, H, a short one, the other, as shown by the dotted line I, a long one, reaching to and resting five or six inches upon the floor. *e e* are small pegs, which, when the bars E are closed, fit into the notches *g g*, so as to stop the bars in the proper place, and prevent their being opened the wrong way. When the bars E are opened, they stretch the tapes K, which are fastened to the tops of the bars *h*, and are about three feet six inches long, to which extent only they suffer the bars to open. When thus extended, and placed in the proper situation, a loose sheet, of the same kind of linen as the curtains, is thrown over, and hangs down over the tapes, and upon the floor at each end, buttoning to the curtains at the corners, so as to form a complete tent, about five feet long, four feet wide, and five feet high, within which both the man and the boy can stand with the machine to work it.

VI.

Abstract of a Paper on the bitter Substances formed by the Action of Nitric Acid on Indigo: by Mr. CHEVREUL *.

Action of nitric acid on indigo examined.

§ I. 1. BEFORE I recite my experiments on the bitter and acid substances, that are obtained by treating indigo with nitric acid, I will briefly advert to the labours of others on the same subject at different periods.

Amer from it, and from silk.

2. Mr. Haussman was the first, who made known the formation of the bitter principle by the action of nitric acid on indigo. Mr. Welther afterward obtained it from silk by means of the same acid, described its principal properties, and gave it the name of *amer*.

Almost all organic substances yield it, and frequently benzoic acid.

3. Messrs. Proust, Fourcroy, and Vauquelin, have shown in several papers, that almost all organic substances, into the composition of which nitrogen enters, yield Welther's amer, and frequently benzoic acid.

Properties of amer according to Fourcroy and Vauquelin.

Messrs. Fourcroy and Vauquelin studied with great attention the properties of the amer obtained with indigo. They observed, that it was acid; and that it was to be considered as a superoxigenated hydrocarburet of nitrogen, forming with pure potash a detonating compound, which appeared not to contain any nitric acid, as Welther had said. They observed farther, that, if the action of the nitric acid on the indigo was stopped, before the whole of the amer was formed, an acid was obtained, which sublimed in white acicular crystals, and appeared much to resemble benzoic acid.

Mr. Hatchett's artificial tannin.

4. Mr. Hatchett, in his learned researches concerning the action of sulphuric and nitric acids on vegetable compounds, made known several products, that precipitate gelatine as tannin does: and on account of this property, combined with several others, he called them artificial tanning matter.

Action of nitric acid on brazil,

5. I observed in the year 1808, that the extract of Brazil wood was converted by nitric acid into a bitter substance,

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* Ann. de Chim. vol. lxxii. p. 113. Read to the National Institute, the 30th of Nov., 1809.

that differed from the amer of Welther: and considered it as a compound of *nitric acid, amer, and artificial tannin.*

and on aloes.

6. Mr. Braconnot, in a paper on gum-resins, speaks of an *acid*, which he obtained with aloes and nitric acid. He remarked, that this acid bore some analogy to the amer of indigo, and also to an orange-coloured substance, that Messrs. Fourcroy and Vauquelin had formed with muscular flesh and nitric acid*.

Supposed acid from indigo.

7. In the month of January, 1809, I resumed my examination of the amer of *Brazil* wood, in order to find how far it resembled the aloetic acid of Mr. Braconnot, when Mr. Vauquelin communicated to me a letter, in which he was informed, that Mr. Moretti, professor of chemistry at Udina, had obtained, by distilling indigo with nitric acid, an acid, that formed a detonating compound with potash, soda, the oxides of iron, lead, silver, &c. It was added, that Mr. Moretti considered it as a new acid, because it could not be confounded with the benzoic, which Messrs. Fourcroy and Vauquelin said they had formed with indigo.

Inquiry instituted concerning it.

Mr. Vauquelin was desirous, that I should repeat these experiments; and at the same time requested me to examine, whether these acid and detonating products did not owe their properties to some nitric acid, which they retained in combination.

Indigo treated with nitric acid.

§ II. 8. Into a retort I poured four parts of nitric acid at 32° [1·283] diluted with four parts of water. A receiver being fitted to it, I placed it on a moderately warm sand heat, and added gradually two parts of Guatimala indigo coarsely pounded. The mixture grew hot, and a quantity of nitrous vapours, carbonic acid, &c., was evolved. Fearing the action would become too violent, I removed the apparatus to a cold sand bath, and left the substances to themselves for four and twenty hours.

Products distilled over.

9. During this time nitric acid, prussic acid, and a small quantity of yellow bitter matter, had passed into the receiver.

Matter in the retort.

10. The liquor, that remained in the retort, was of a reddish yellow, and two concrete substances floated on it.

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* See Journal, vol. xxvii, p. 361.

The most abundant had the appearance of a *resin*. The other was of an orange-colour, and disseminated in this in the form of clots. These were both separated from the liquid, washed with cold water, and then boiled. The resinous matter congealed on cooling; and the orange-coloured substance, after having dissolved, fell to the bottom in small grains, which did not adhere to each other.

The liquid distilled.

Products.

Yielded crystals of amer and an acid.

11. The water, that had been employed to separate these two substances, was added to the liquor (10) left in the retort, and then distilled. Nitric acid, prussic acid, amer, and a little ammonia, passed into the receiver. The concentrated liquor on cooling let fall crystals formed of the *amer* of Welther, and of the *benzoic acid* of Messrs. Fourcroy and Vauquelin. Having dissolved these in hot water, I obtained by cooling the *crystallized acid*, retaining a little amer; and by evaporating the liquid fine yellow scales of amer.

Fat oil from the mother water.

12. The liquid, which had furnished the crystals (11) of amer and acid, after boiling down let fall a *red liquid substance resembling a fat oil.*

More of this, and oxalate of lime.

13. The supernatant liquor (12) was evaporated to dryness, and hot water poured on the residuum. *Oxalate of lime* was left undissolved; and the water, on cooling, let fall some oily matter, and afterward a *yellow sediment, which was pretty soft*, and differed from the oily matter only in the proportion of its principles.

The products examined.

14. I shall now proceed to examine, 1st the amer; 2d, the acid substance, which has been compared to benzoic acid; 3d, the resin. The other products being only compounds of these three, I shall not speak of them under separate heads; but I intend in a future paper to return to the *substance of an oily appearance.*

§ III. Art 1. *Of the Amer.*

The amer impure

15. The scales of amer, which I mentioned (11), retained a little resin, whence they derived a deep yellow colour; and a small quantity of the acid, which has been called the benzoic, but which I shall designate under the name of *volatile acid.*

Its properties when pure.

When the amer is very pure, it is white inclining to straw-colour. Its solution in water is not reddened by salts of iron at a maximum. That which was employed in the following experiments had been boiled in nitric acid: afterward crystalized repeatedly; combined with potash, and then separated from it by muriatic acid; and lastly crystallized, till, when redissolved in water, it no longer precipitated solution of silver.

Action of heat on it.

16. The amer, being gently heated in a common phial, sublimed in little needles, or scales, of a white colour inclining to straw-yellow. Thrown on a redhot iron it took fire, and left a coal, which melted. If exposed to a red heat in a retort, a pretty strong smell of nitrous acid and of prussic acid is evolved.

Apparatus for examining its products.

To examine the products of the amer subjected to the action of heat, I contrived an apparatus consisting of a glass ball surmounted with a tube, which terminated under a jar filled with mercury. Into the ball I introduced 2 dec. [3 grs.] of amer, more would have burst it, and fastened the tube to the jar by means of a wire.

Heat applied.

17. When the apparatus was thus arranged, I heated the ball with a redhot coal: the amer melted, grew black, and took fire; a light coal remained; and aqueous vapour, gas, and a little charcoal passed into the receiver.

Gaseous products.

The gaseous product reddened litmus paper. It had the smell of nitrous acid, mixed with that of prussic; and I analysed it in the following mode. I first passed some water into the jar, and a slight absorption took place. When this appeared to be at an end, I shifted the water to another jar filled with mercury; and found, that it had dissolved a portion of amer, which had been volatilized without decomposition, some carbonic acid, and some prussic acid. To detect the latter it was necessary, to saturate the liquid with carbonate of potash; and pour it into a small glass retort adapted to a receiver, in which were some threads twisted together, impregnated with green sulphate of iron, and afterward dipped in a weak solution of potash. On distilling, water and prussic acid passed over; and the thread, after having been washed with weak muriatic acid, became blue. (If sulphate of iron

Carbonic and prussic acids.

Mode of detecting the latter.

were mixed directly with the liquid saturated with potash, no prussian blue was obtained).

Residuum.

The residuum of the distillation was reddish, and gelatinized. Sulphuric acid evolved from it a smell of prussic acid, mixed with that of nitrous acid.

Nitric acid and a little ammonia.

The water therefore had dissolved, beside the undecomposed amer, *carbonic acid*, *prussic acid*, and *nitric acid*: and I have every reason to think, that it contained a little *ammonia*.

The gaseous products not soluble in water were

18. The gaseous residuum insoluble in water was placed in contact with a solution of potash for twenty-four hours, in order to abstract from it the carbonic and prussic acid, that it might still retain. After this it was carefully washed. In this state it did not redden infusion of litmus; but as soon as it was exposed to the air, it reddened it, and produced a pretty strong smell of nitrous acid; so that it must have contained *nitrous gas*. It burned in the same manner as *oily hidrogen*. I had observed several times, that this residuum extinguished burning substances, because then it contained a great deal of nitrogen gas; and at other times, that it burned like carbonic oxide gas. It appeared, that these differences were owing to the degree of rapidity, with which the amer was decomposed.

nitric oxide, hidrogen, and nitrogen.

The gaseous residuum insoluble by water or potash, therefore, consisted of *nitrous gas*, *inflammable gas*, and *nitrogen*. As I was not able to make an accurate analysis of this residuum, I cannot say whether the whole of the nitrogen came from the air in the apparatus, the oxigen of which was converted into nitric acid by the nitrous gas; or whether part of it arose from the decomposition of the amer.

Composition of amer.

19. From these facts it appears to me, that *amer* is a compound of *nitric acid*, and a *vegetable matter*, probably of an oily or resinous nature. Perhaps it may be objected, that the nitrous gas obtained may be formed during the process, by the compression the gasses undergo: as is the case when hidrogen is detonated with oxigen containing nitrogen in Volta's eudiometer, or in a glass globe for the recomposition of water: but the compression of the gasses in these apparatuses appears to me to be much greater, than

they experience in the detonation of amer. However, the facts that follow, and those which I purpose to relate, will show, that it is more natural to consider amer as a compound of nitric acid, than as a substance formed directly of oxigen, hidrogen, carbon, and nitrogen.

Action of potash on amer.

20. Amer is much more soluble in hot water than in cold. Its solution is acid, very bitter, and even a little astringent. If it be mixed with a concentrated solution of potash, small needly crystals of a gold colour are obtained, which are a compound of amer and potash, and have been described by Welther, Fourcroy, and Vauquelin. These crystals detonate loudly when heated. They cannot be heated in a glass ball without breaking it to pieces. If 15 cent. [2 grs.] be heated in a small assay matrass, a loud detonation is produced, the vessel is filled with soot, and a smell of prussic acid is emitted. If the matrass be closed as soon as the detonation has taken place; and, when it is cool, a solution of potash be poured in, and afterward of green sulphate of iron, prussian blue will be obtained.

A detonating compound formed.

Dissolved in water.

100 dec. of boiling water dissolved 7 dec. of the detonating matter. On cooling, a great part of the latter separated in the form of small needles. The solution was not acid, and did not appear alkaline.

Properties of this compound.

This compound is decomposed by nitric or muriatic acid at a boiling heat, as Messrs. Fourcroy and Vauquelin observed; and, on cooling, the amer crystallizes in white scales, inclining to straw-colour. But a very remarkable fact, which proves, that there is no such thing as elective attraction, is: if you take a solution of potash, supersaturated with nitric or muriatic acid, mix with it amer, evaporate to dryness in a small capsule, and dissolve the residuum in hot water, you will obtain on cooling small detonating crystals, of a gold colour, formed of amer and potash; whence it follows, that amer decomposes nitrate and muriate of potash.

No such thing as elective attraction.

Causes of these opposite effects.

Thus we have two opposite effects, that occur within a range of temperature the extremes of which are by no means remote; and which are easily explicable, if we attend to the circumstances. In the first, we perceive that the amer must first separate, since potash is capable of forming with the nitric or muriatic acid a compound more soluble in water than amer: the latter therefore is separated by the force of crystallization. In the second experiment, the amer and potash, being more fixed than the nitric or muriatic acid, combine; while the acid flies off by the expansive power of heat.

Union of amer with ammonia,

Amer combines very well with ammonia, and the result is small yellow scales, which scarcely detonate on being heated.

the earths,

21. Amer unites with lime, barytes, and strontian, and forms compounds soluble in water. It requires but a very small quantity of lime, or even of carbonate of lime, to turn the crystals of pure amer yellow. The mere contact of common paper is sufficient, to produce this effect.

oxide of silver,

22. Amer dissolves oxide of silver, and forms with it needles of a rich gold colour; but they grow black by exposure to air. A similar compound may be obtained by pouring amer into a solution of silver, and leaving it to evaporate spontaneously.

carbonate of lead,

It dissolves carbonate of lead, and forms with it acicular crystals, which are not very soluble, unless they contain an excess of amer.

and oxide of mercury.

It equally dissolves red oxide of mercury.

All these compounds detonate when heated.

Theory of its detonation.

23. The theory of the detonation of amer is easily comprehended. When its temperature is raised, part of the nitric acid is converted into nitrous gas. The other and greater part is wholly decomposed; the oxigen of the acid attacks the combustible principles of the vegetable matter, and forms water with the hidrogen, and carbonic acid with the carbon; the whole, or part, of the nitrogen of the nitric acid forms prussic acid, and perhaps ammonia, with some of the hidrogen and carbon; another portion of hidrogen, uniting with carbon, forms oily inflammable gas. The residual coal is so much the more bulky, in proportion as the amer has been less strongly heated, because then the most dilatable principles are first evolved.

Fixity of the nitric acid in amer.

One thing to be observed in the action of a gentle heat on amer is the fixedness, that the constituent principles of nitric acid appear to have acquired in this compound: for we find, that combustion does not take place, till the charcoal is predominant.

This confirms the opinion of forming a chemical union with it.

This fact appears to confirm the existence of nitric acid in amer; because it seems, that the oxigen, if it were combined directly with the carbon, hidrogen, and nitrogen, would attack the hidrogen, as soon as the combination between the principles of the amer was loosened by heat, rather than remain attached to the carbon, waiting till the temperature of the latter was sufficiently raised, to admit its combining with it*. On the hypothesis, that amer is a compound of nitric acid with a combustible formed of hidrogen and carbon †, we can better understand what passes, on heating it gently. In this case, part of the hidrogen, a little carburetted, is evolved at a temperature too low to separate the principles of the nitric acid, so that the nitric acid, when it has reached the temperature at which this separation is effected, acts on a combustible, that has already lost a part of its hidrogen, and consequently finds itself more carburetted than it was before.

The detonating power of amer

The detonation of amer ought to be loud, because it contains oxigen enough to saturate the greater part of its combustible elements, and form gaseous compounds with them: but, as it is volatile, it follows, that one portion escapes combustion; and that this takes place successively, because the heat is not uniform. Thus we can account for the effect produced by an alkaline base, when it is combined with amer, and when this compound is made to detonate. In this case, the amer, being rendered more fixed, becomes much more detonating; because, as the heat is allowed to accumulate, its elements separate simultaneously, and thus produce a more forcible detonation. This is the way in which Messrs. Fourcroy and Vauquelin view the action of the base; and in proof of its truth it is to be observed, that the detonation is in general so much the stronger, in proportion as the base, with which the amer is combined,

increased by a fixed alkali,

and still more by a metallic oxide.

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* It is well known, that, in almost all cases, where hidrogen united with other combustible substances is present with oxigen, the oxigen attacks the hidrogen preferably to the others.

† Containing perhaps a little oxigen and nitrogen.

combined, is more fixed. Thus the compound of potash with amer detonates more loudly than that of ammonia, and less than that of oxide of lead. But there are various circumstances capable of modifying the strength of the detonation: 1st, the quantity of amer united with the base: 2d, the force with which they are combined: and 3d, the nature of the base. Thus, for instance, the metallic oxides that are easily reducible form compounds, that detonate more feebly than those that are difficult of reduction.

Circumstances that modify the detonation.

Amer precipitates gelatine.

24. The solution of amer precipitates isinglass: but the precipitate is soluble in an excess of gelatine, and in all the acids. This property I shall notice in a subsequent paper.

§ III. Art. 2. *Of the Volatile Acid.*

The orange-coloured matter a compound.

25. The orange-coloured matter separated from the resin by boiling water (10) was little soluble in cold water. Boiling water separated some resin from it; and the crystals, that fell down on cooling, were fawn-coloured. These crystals were composed of *volatile acid*, *resin*, and a small quantity of amer. I purified it in the following manner. I dissolved 5 gram. [77 grs.] in hot water, added in five portions as many grammes of carbonate of lead, boiled and filtered. On the paper was left a yellow powder, consisting of resin and a portion of volatile alkali combined with oxide of lead. To the filtered liquor I added sulphuric acid; and the oxide of lead (with which the volatile acid had combined, forming with it a soluble compound with excess of acid) fell down in the state of sulphate. The liquor I filtered still boiling, evaporated, and obtained by refrigeration white acicular crystals, united by their extremities in the form of stars. Having left them to drain, I redissolved them in boiling water, and thus separated a small quantity of resin; when the crystals obtained by cooling were of a fine white like wax. To obtain these crystals in all their whiteness, care must be taken not to dry them on paper that contains carbonate of lime, or oxide of iron, as this would turn them yellow, or reddish.

The volatile acid purified.

More obtained from the mother water.

On boiling down the mother water, that had yielded the crystals of volatile acid, more crystals of volatile acid were deposited, which were tinged yellow by a little resin; and afterward a *substance of an oily appearance*, which was composed of volatile acid, amer and resin.

Properties of this substance.

26. The crystals of volatile alkali have a taste slightly acid, bitter, and astringent.

Thrown on redhot iron, one part is volatilized, another is decomposed, and leaves a coal that fuses.

They may be sublimed in white needles, by heating them gently in a phial.

Decomposed by heat.

Heated in the glass ball already described (16), they melt; part is volatilized into the jar; and what remains in the ball grows black, and leaves a bulky coal, which is slightly fusible. Much less gas is formed than in the detonation of amer.

Gaseous products.

The gasses produced in this experiment have an empyreumatic vegetable smell. Distilled water absorbs more than three fourths, and then appears to contain nothing but *carbonic acid*, except the undecomposed portion of the volatile acid. I could not obtain any prussic acid from it by distillation. The gaseous residuum insoluble in water and potash consists of *nitrogen*. I had not enough to determine, whether it contained any hidrogen.

The acid more soluble in hot water than in cold.

27. The volatile acid is far more soluble in hot water than in cold. The solution has a very slight yellow colour. It reddens litmus paper. It does not precipitate gelatine like amer; and differs from it in giving a fine red to all the salts of iron at a maximum, but it does not change the colour of the salts at a minimum.

Nitric acid converted it into amer.

28. Nitric acid at 40° [sp. gr. 1 386] boiled on the volatile acid converted it into Welther's amer. Muriatic acid appeared to have no action on it.

It is soluble in solution of potash.

29. The volatile acid dissolves very well in solution of potash; or in its carbonate, if assisted by heat, and carbonic acid is evolved. On boiling down this solution, which is of an orange colour, small red acicular crystals are formed. These are much more soluble in water than the compound of amer and potassium, are much less bitter, and do not detonate, but swell when placed on a redhot iron. On exposing them to the action of heat in the glass ball, first a yellow vapour was disengaged, after which they melted,

The compound decomposed by heat.

without emitting much light. An alkaline coal remained, retaining *carbonic acid* and *prussic acid.* There was also a little undecomposed volatile acid.

Gaseous product.

The gaseous product consisted in great part of carbonic acid and nitrogen.

The acid combined with the earths,

30. Lime, barytes, or strontian water, gives the solution of volatile acid a fine yellow colour. On boiling down the compound of the acid and barytes, small orange-coloured crystals are formed by cooling, which, when exposed to heat, do not detonate, but grow red hot, and afterward leave a coal, that throws out a number of little red sparks as it burns.

All these compounds are decomposed by sulphuric, nitric, or muriatic acid.

The volatile acid did not appear to me to decompose the muriate or nitrate of potash like amer.

oxide of silver,

31. The volatile acid boiled with oxide of silver dissolves it; but, if the solution be boiled too long, it grows black, and the oxide of silver appears to be reduced.

oxide of lead,

Assisted by heat it decomposes carbonate of lead, dissolves part of the oxide, and deposits, on cooling, small orange-coloured crystals, which melt without detonating.

and oxide of iron.

It dissolves red oxide of iron, and acquires a hyacinthine red colour.

All these compounds appeared to me to be acid.

§ III. Art. III. *Of the resinous Matter.*

Purification of the resinous matter.

32. The resinous matter, which had been separated from the orange-coloured matter by boiling water (10), was subjected anew to the action of this agent, till the water came off very slightly coloured. This required a considerable time. The insoluble residuum was treated repeatedly with boiling water, the resin was dissolved; and a mixture of oxalate of lime, sand, &c. remained.

Its properties.

33. The resin was separated from the alcohol by adding water, and the liquid evaporated. The resin thus obtained was brown, very slightly bitter, and gave a faint yellow tinge to the water in which it was boiled. This water did not precipitate gelatine, and did not give a red colour to sulphate of iron, but threw down with it a slight precipitate.

It did not redden litmus paper, till it was concentrated by boiling.

When the resin was thrown on a red hot iron, it emitted a fragrant smell, and left a tumid coal.

Distilled.

On distilling it in a small glass retort, I obtained, among other products, a liquid, that had a strong smell of prussic acid, and of oily ammonia.

Retains nitric acid in combination.

The resin is soluble in solution of potash, nitric acid, and alcohol. I believe, that, even after it has been well washed, it contains nitric acid in real combination, and a little volatile acid and amer. It is probable, that all the resinous matters, which are formed in treating animal or vegetable compounds with nitric acid, retain a portion of this acid in combination.

Impure part soluble in water.

34. The waters in which the resin was washed (32) were reddish, grew turbid on cooling, and let fall a resinous matter, which was a little viscous, bitter to the taste, and appeared to differ from the washed resin only in containing a larger quantity of nitric acid, volatile acid, and amer. I imagine it was by means of these only, that it dissolved in the water.

On treating this resinous matter with one fourth its weight of carbonate of lead, I obtained a solution of amer, volatile acid, and lead. The resin was not dissolved, but retained a little oxide of lead in combination with it.

If the resin be boiled with an excess of carbonate of lead, it is still acid, and still yields ammonia by distillation. This confirms me in the opinion, that it retains nitric acid.

The resin treated with nitric acid.

35. The resin, when treated repeatedly with nitric acid at 45° [sp. gr. 1·455], was dissolved into a reddish brown liquor. From this water separated a substance of the yellow colour of shamoy leather, which appeared to me to be resin combined with amer and nitric acid. Some amer, matter of an oily appearance, and a little resin, remained in solution.

Amer thus formed from it.

Though the resin, that had been employed in my experiments, contained a little amer, I have no doubt, that a certain portion was formed by the action of the nitric acid, as Messrs. Fourcroy and Vauquelin have said. What appeared to me, to prevent the total conversion of the resin into amer, is the combination that takes place between these two products. I have even observed, that this combination is capable, in a certain degree, of defending the volatile acid from the action of the nitric acid.

§ IV. *Reflections on the Nature of the Volatile Acid and of the Amer.*

Nature of the amer and the volatile acid

36. If the amer be considered as a compound of nitric acid and a vegetable substance, the nature of which is yet unknown, we shall be led to regard the volatile acid as a similar compound, differing from the former only in containing less nitric acid. I am fully aware, that I cannot prove these assertions by direct experiments; but, if we compare the facts mentioned in this paper, we shall see that they have a great appearance of truth.

1st. The volatile acid and its compounds, when exposed to heat, comport themselves nearly in the same manner as amer and its compounds. The different quantities of nitric acid they contain explain why the former only melt, while the latter detonate forcibly.

2d. Nitric acid boiled with the volatile acid converts it into amer.

3d. The compounds of amer have a great resemblance to those of the volatile acid. Their taste is more or less bitter, their colour a yellow, more or less deep.

4th. If it be true, that the matter combined with the nitric acid is of an oily nature, we can conceive, why the amer is more soluble in water than the volatile acid, which contains less nitric acid; why the volatile acid is more soluble in alkalis than in water; and why the compounds of amer are lighter coloured than those of the volatile acid.

If it be asked, why the volatile acid, in its decomposition by heat, gives out no nitrous gas, while the amer does; I answer, there are two reasons why the nitric acid of the former should be radically decomposed: first, because the combustible elements in the former are in larger proportion than in the amer; secondly, because the nitric acid is more strongly retained by it.

37. If these reasonings be just, the volatile acid may be termed *amer with a minimum of nitric acid;* and Welther's amer, *amer with a maximum of nitric acid.*

It remains for farther experiments to show, whether it be possible to separate the nitric acid from these substances, without employing the assistance of heat; and whether the amer at a minimum be not a compound of amer at a maximum, already formed, with an oily or resinous matter.

I intend to subject these substances to the action of the voltaic pile as soon as leisure will permit me.

The detonating substance not a compound of amer and nitre.

38. Mr. Welther, in his paper on amer, considered the detonating substance it forms with potash as a compound of nitrate of potash and amer. This opinion was founded on the following experiment, which is very accurate. He took crystallized amer, mixed it with nitrate of potash, evaporated, and obtained the detonating substance. But I have shown above, that the detonating substance is formed with muriate of potash, as well as with nitre; and that consequently the acid of the nitre had no influence in the production of the detonating substance.

Welther's view of it different from the author's.

This experiment shows at the same time the difference there is between the view I have taken of amer in the course of this paper, and that of Mr. Welther, in his experiments on silk. In all the experiments I have described, it may have been observed, that the amer did not yield its nitric acid to any substance, that it acted on the bases by a resulting affinity, and that consequently nitric acid was a principle necessary to its existence; while Mr. Welther considered amer as a substance *sui generis*, which became detonating only by combining with nitrate of potash.

Moretti's new acid nothing but amer.

39. The new acid, which Mr. Moretti speaks of having obtained by distilling indigo with nitric acid (7), appears to be nothing but amer at a maximum; at least the properties he ascribes to it belong to the latter compound.

VII.

Analysis of Hedge Hyssop, Gratiola Officinalis, of the Order Bignonia of Jussieu: by Mr. VAUQUELIN *.

Object of the experiments.

THE experiments, of which I am about to give an account, were instituted for the purpose of ascertaining the nature of the purgative principle of hedge hyssop.

The expressed juice.

1. The expressed and filtered juice of this plant has but little colour, compared with that of many other vegetables. Its taste is acrid and bitter. It is rendered very slightly turbid, by heat, or by aqueous infusion of galls, which indicates, that it contains but a very small portion of animal matter. It has also but little acidity †.

Distilled.

2. Subjected to distillation, it yielded a water void of taste, and in which nothing was detected by a considerable number of tests. It contains, therefore, no principle, that is volatile at the temperature of boiling water.

Extract treated with alcohol.

3. The juice being evaporated to the consistence of an extract, and treated with alcohol, the greater part dissolved in it. That which did not, was higher coloured than the other, and insipid, or with very little taste: a proof, that this property pertains to the part soluble in alcohol. The alcoholic solution, being evaporated to dryness, left a brownish yellow substance of extreme bitterness.

Alcoholic extract treated with water

This substance, being treated with water, imparted to it a pretty deep brown colour, and a bitter taste; leaving a soft substance, drawing out into strings like a resin, and the taste of which, at first sweetish, was afterward extraordinarily bitter. Though this substance appeared to be insoluble in water, yet it dissolves in a large quantity of this fluid when heated.

Two substances extracted by the alcohol.

The alcohol therefore had dissolved two substances from the extract of hedge hyssop, a resin, and a matter soluble in water. Beside the bitter taste of the latter, it has considerably pungency, owing to the salts it contains, the nature of which will be made known below.

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* Ann. de Chim. Vol. LXXII, p. 194.

† In plants that contain an acrid principle there is generally little or no animal matter; as if these substances were incompatible, or the circumstances favourable to the formation of the one were not suitable for the formation of the other.

The resin made more soluble by the salts.

It appears to be these salts, that communicate to the resin the faculty of dissolving in water more abundantly: for, once divested of them, it is much less soluble in this fluid.

Action of reagents on the solution.

This resinous substance, while dissolved in water, was exposed to the action of various tests, which exhibited the following effects:

1. Oxalate of ammonia rendered the solution slightly turbid.

2. Nitrate of barytes produced no change.

3. Muriate of platina formed a small quantity of a triple salt.

4. Nitrate of silver threw down a very copious yellow precipitate, part of which was soluble in nitric acid, and the remainder had all the appearance of muriate of silver.

5. With acetate of lead it gave a brownish precipitate, completely soluble in nitric acid.

6. Litmus paper was pretty strongly reddened by it.

Calcined.

7. Part being evaporated, and calcined in a platina crucible, exhaled a very acrid and pungent matter; after which it was converted into a very bulky coal, with a taste somewhat alkaline.

The coal lixiviated.

The lixivium of this coal, being evaporated, yielded crystals, that tasted like muriate of soda. These crystals, when treated with dilute sulphuric acid, effervesced briskly; which proves, that they were mixed with an alkaline carbonate. The solution of these crystals, evaporated to dryness, calcined, and redissolved in water, yielded sulphate of soda, mixed with a small quantity of sulphate of potash.

Salts.

These results prove, that hedge hyssop contains muriate of soda, and another salt with base of potash, the acid of which is of the vegetable kind, since it was destroyed by heat, and leaves carbonic acid in its stead.

It is to be presumed, that this acid is the malic, or the acetic; for the salt composed of it was dissolved by alcohol, and its solution, even when greatly concentrated, afforded no nitrate of potash: the only salt besides, that would have dissolved in alcohol, and been decomposed by heat.

The

Matter insoluble in alcohol examined.

The matter insoluble in alcohol, that has been mentioned above, had no taste, and was redissolved entirely by water, to which it gave a viscous consistence, as a gum would have done.

To satisfy myself whether it were really a gum, I digested it in hot nitric acid, which soon took away its colour, and dissolved it. The solution only remained of a light yellow.

Treated with nitric acid.

When thus treated by nitric acid, it yielded a white flocculent substance, insoluble in water, which I took at first for mucous acid; but farther experiments led me to consider it as a mixture of this acid and oxalate of lime.

White powder separated.

To obtain this white powder separate, I decanted the supernatant yellow liquid, and afterward washed the residuum with small portions of cold water, till it was very white; when I separated it by the filter, and dried it.

Its properties.

This powder had a slight acid taste; and, being diluted with a little water on litmus paper, it reddened it perceptibly. Placed on a burning coal, it swelled up, grew black, and emitted a smoke that smelt exactly like that of sugar treated in the same manner. In ammonia it appeared at first to dissolve; but soon after a flocculent substance formed in the liquid.

Having filtered the ammoniacal solution, I added a few drops of nitric acid, to saturate the alkali; and see whether the mucous acid, being little soluble, would not fall down: but the mixture remained perfectly clear. To the same solution I added lime-water, till the slight excess of nitric acid it contained was saturated; but no precipitation took place. Lastly, I mixed a pretty large quantity of alcohol with the ammoniacal solution, which produced in it no change.

From these experiments it appears, that the ammonia, with which I treated the white powder in question, dissolved no oxalic acid; since lime-water, added to it, did not render it turbid: and also, that it dissolved no mucous acid, since I could not cause any to reappear.

Mucous acid.

Yet it must have dissolved something; for the white powder was reduced to a very small quantity: and I am inclined to believe, both from the external appearance of the substance, and from the smell of burnt sugar it emitted when placed on a live coal, that it could be nothing but the mucous acid, notwithstanding I found it impossible to demonstrate it.

Oxalate of lime.

As to the substance that was not dissolved by the ammonia, I satisfied myself by various experiments, that it was oxalate of lime.

Malate of lime.

The presence of lime in this white powder indicates, that the alcohol had precipitated with the gum some malate of lime, which in fact is not soluble in that liquid. The yellow liquor decanted from the white powder contained also oxalate of lime in solution, free oxalic acid, and yellow bitter matter: for ammonia threw down from it a white granular precipitate; the filtered liquor was afterward rendered turbid by lime-water; and the solution still remained yellow and bitter.

The mucous matter.

The mucous matter of hedge hyssop, which was separated from the bitter principle by means of alcohol, as said above, contained therefore lime in combination with an acid; and probably a small quantity of vegeto-animal matter, which formed the yellow bitter matter by the alteration it underwent in the nitric acid.

The green resin

The green resin of hedge hyssop exhibited nothing peculiar. Like that of other vegetables, it is soluble in alcohol, in alkalis, and in fats.

Soluble principles of hedge hyssop.

The experiments I have made on hedge hyssop, the chief of which I have here related, show, that this plant contains the following soluble principles, which are consequently found in its expressed juice: 1, a gummy matter of a brown colour: 2, a resinous matter; which differs however from most resins in being soluble in a large quantity of water, particularly when heated; much more soluble in alcohol than in water, and of an extremely bitter taste: 3, a small quantity of animal matter: 4, muriate of soda in pretty large quantity: and, 5, a salt with base of potash, which I suspect to be a malate. I detected the existence of this salt by means of solution of platina, and simple sulphate of alumine.

Solubility of the resin.

It appears, that the solubility of the resin is increased by the presence of the gummy matter, and of the salts; for, when it is freed from these, it can no longer be dissolved in water;

water in so large proportion as it exists in the juice of the plant.

Its properties. The consistence of this sort of resinoid is that of a soft paste: but if it be exposed some time to the open air, it dries, and becomes friable. Its extraordinarily bitter taste has much resemblance to that of colocynth, though the plant that furnishes it is not of the same family: it differs from it however by a saccharine taste preceding the bitter.

Solid substance of the plant examined. After having expressed the juice of the hedge hyssop, and exhausted the magma by water and alcohol, I left it three days in diluted nitric acid. I then pressed the acid strongly through a cloth, washed the magma with water, and added ammonia to the united liquors, in which it formed a flocculent yellow precipitate. As this precipitate showed some traces of vegetable matter, I calcined it lightly; after which it dissolved with effervescence in muriatic acid, and from the solution ammonia threw down a yellow precipitate which consisted of phosphate of lime, and oxide of iron. It also yielded, on the addition of oxalic acid, a certain quantity of oxalate of lime.

The magma of the hedge-hyssop still contained oxalate of lime, phosphate of lime, and iron, which also was probably united with phosphoric acid.

Lastly, the magma, having been burned, left ashes consisting for the most part of silex, with a little calcareous earth and iron.

The active principle. From what has been said there appears no doubt, that the active and cathartic principle of hedge hyssop is the substance soluble in alcohol, which I have called a resinoid; since it is the only one in it, that has any taste. Its solubility in water, which is increased by the gum and the salts that accompany it, explain why the infusion, and still more the decoction of the plant, are purgative, and even drastic.

Formerly prohibited, for its violence. The violent action of hedge hyssop on the animal economy has long been known to physicians; and this no doubt is the reason, why the sale of this plant has formerly been prohibited. This prudent measure, which has fallen into disuse, ought to be revived; for accidents frequently happen from the ignorance of herbalists, or of those who use this plant, respecting its virtues.

Instance of its danger.

To mention only one recent instance: a man complaining of a pain in the loins, where one of those good women were present, who act gratuitously as physicians, and have a remedy for every disease; she advised him to have recourse to clysters of a decoction of hedge hyssop. Unfortunately he too readily followed the prescription; for, a few moments after the remedy was administered, he was seized with violent griping pains, and these were succeeded by evacuations of blood, which continued for more than a week; and, but for the assistance of a skilful physician, who ought to have been consulted before, he would probably have lost his life.

VIII.

On the Causes which influence the Direction of the Growth of Roots. By T. A. Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S*[*].

Growth of radicles.

I HAVE shown, in a former communication, the effects of centrifugal force upon germinating seeds; from which I have inferred, that the radicles are made to descend towards the earth, and the germes, or elongated plumules, to take the opposite direction, by the influence of gravitation; and I believe the facts I have stated to be sufficient to support the inferences I have drawn[†]. But the fibrous roots of plants, being much less succulent, though not uninfluenced in the directions they take by gravitation, are, to a great extent, obedient to other laws, and are generally found to extend themselves most rapidly, and to the greatest length, in whatever direction the soil is most favourable: whence many naturalists have been disposed to believe, that these are guided by some degrees of feeling and perception, analogous to those of animal life.

Fibrous roots obedient to other laws;

whence supposed to have perception.

[*] Phil. Trans. for 1811, p. 209.

[†] Phil. Trans. 1806, page 5: or Journal, vol. xiv, p. 409.

I shall

Facts on which this opinion is founded.

I shall proceed to state some of the facts, upon which this hypothesis has been founded; and others, which have occurred in the course of my own experience, and which are favourable to it; after which I shall endeavour to trace the effects observed to the operation of different causes.

Roots tending to or from water

When a tree, which requires much moisture, has sprung up, or been planted, in a dry soil, in the vicinity of water, it has been observed, that much the largest portion of its roots has been directed towards the water; and that when a tree of a different species, and which requires a dry soil, has been placed in a similar situation, it has appeared, in the direction given to its roots, to have avoided the water and moist soil.

Roots from a tree on a wall.

A tree growing upon a wall, at some distance from the ground, and consequently ill supplied with food and water, has also been observed to adapt its habits to its situation, and to make very singular and well directed efforts to reach the soil beneath, by means of its roots *. During the period in which it is making such efforts, little addition is made to its branches, and almost the whole powers of the plant appear to be directed to the growth of one or more of its principal roots. To these much is in consequence annually added, and they proceed perpendicularly towards the earth, unless made to deviate by some opposing body: and as soon as the roots have attached themselves to the soil, the branches grow with vigour and rapidity, and the plant assumes the ordinary habits of its species.

Tree planted in the intersection of two trenches

Du Hamel caused two trenches to be made so as to intersect each other at right angles, and a tree to be planted at the point of intersection; and taking up this tree some years afterward, he found that the roots had almost wholly confined themselves to the trenches, in which the soil of the former surface must have been buried.

Carrots and parsneps sown on a poor soil with a rich substratum:

A trench, which was twenty feet long, six wide, and about two deep, was prepared in my garden, in the bottom of which trench was placed a layer, about six inches deep, of very rich mould, incorporated with much fresh vegetable

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* Smith's Introduction to Botany.

matter. This was covered, eighteen inches deep, with light and poor loam; and upon the bed thus formed seeds of the common carrot (*daucus carota*) and parsnep (*pastinaca sativa*) were sowed. The plants grew feebly till near the end of the summer, when they assumed a very luxuriant growth, grew rapidly till late in the autumn, and till their leaves were injured by frost. The roots were then examined, and were found of an extraordinary length, and in form almost perfectly cylindrical, having scarcely emitted any lateral fibrous roots into the poor soil, while the rich mould beneath was filled with them.

others in a rich soil with a poor substratum.

In another experiment of the same season, the preceding process was reversed, the rich soil being placed upon the surface, and the poor beneath. The plants here grew very luxuriantly, and acquired a considerable size early in the summer; and when the roots were taken up in the autumn, they were found to have assumed very different forms. The greater part had divided into two or more unequal ramifications, very near the surface of the ground, and those which were not thus divided tapered rapidly to a point at the surface of the poor soil, into which few of their fibrous roots had entered.

Plants growing so as to select either soil.

In other experiments seeds of almost all the common esculent plants of a garden were so placed, that the young plants had an opportunity of selecting either rich or poor soil; which was disposed, in almost every possible way, within their reach; and I always found abundant fibrous roots in the rich soil, and comparatively few in the poor.

Beans placed so as to be exposed to the air beneath them and to earth above them.

The following experiment afforded the most remarkable result, and one the least favourable to the hypothesis, which I have advanced in a former paper *, and to the conclusion which I shall now endeavour to support; and therefore I think it necessary to describe it very minutely. Some seeds of the common bean (*vicia faba*), the plant with which many former experiments were made, were placed upon the surface of the mould in garden pots, in rows which were about four inches distant from each other. A grate, formed of slender bars of wood, was then adapted to the surface of each pot, so as to prevent both the mould and the seeds falling out, in whatever position the pots might be placed: and the bars were so disposed, as not at all to interfere with the radicles of the seeds, when protruding. The pots were then directly inverted: and the seeds were consequently placed beneath the mould; but each seed was so far depressed into the mould, as to be about half covered: by which means each radicle, when first emitted, was in contact with the mould above, and the air below. Water was then introduced through the bottom of the inverted pot, in sufficient quantity to keep the mould moderately moist; and, the pots being suspended from the roof of a forcing house, the seeds soon vegetated.

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* Phil. Trans. 1806, page 1; or Journal, vol. xiv, p. 409.

The radicles extended horizontally, and shot up fibres into the earth.

In former experiments*, wherever the seeds were placed to vegetate at rest, the radicles descended perpendicularly downward, in whatever direction they were first protruded; but under the preceding circumstances they extended horizontally along the surface of the mould, and in contact with it; and in a few days emitted many fibrous roots upward into it: just as they would have done, if guided by the instinctive faculties and passions of animal life; and as I concluded before I made the experiment that they would do, under the guidance of much more simple laws, the mode of operating of which I shall endeavour to explain.

Ascending sap first set in motion by the stems and branches, and the quantity replaced by their ability to employ it, to which light is conducive.

Whatever be the machinery, by which the sap of trees is raised to the extremities of their branches, it is obvious, that this machinery is first put into action by the stems and branches, and not by the roots; for the graft or bud, whenever it has become fully united to the stock, wholly regulates the season and temperature, in which the sap is to be put in motion, in perfect independence of the habits of the stock; whether these be late or early. If all the branches of a tree, exclusive of one, be much shaded by contiguous trees †, or other objects, the branch which is exposed to the light attracts to itself a large portion of the ascending sap, which it employs in the formation of leaves and vigorous annual shoots, while the shaded branches become languid and unhealthy. The motion of the ascending current of sap appears therefore to be regulated by the ability to employ it in the trunk and branches of the tree; and this current passes up through the alburnum, from which substance the buds and leaves spring. But the sap, which gives existence to, and feeds the root, descends through the bark[*]: and if the operation of light give ability to the exposed branch to attract and employ the ascending or alburnous current of sap, it appears not improbable, that the operation of proper food and moisture in the soil, upon the bark of the root, may give ability to that organ to attract and employ the descending, or cortical current of sap; and if this be the case, an easy explanation of all the preceding phenomena immediately presents itself.

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* Phil. Trans. 1806, p. 1: or Journal, vol. xiv, p. 409.

† Phil. Trans. 1805 and 1809: or Journal, vol. xii, pp. 233, 308; vol. xxv, p. 118.

Proper food enables the root to attract and employ the descending sap.

Descent of roots from a tree on a wall accounted for.

A tree growing upon a wall, and unconnected with the earth, will almost of necessity grow slowly, and as it must be scantily supplyed with moisture during the summer, it will rarely produce any other leaves, than those which the buds contained, which were formed in the preceding year. Some of the roots of a tree, thus circumstanced, will be less well supplied with moisture than others, and these will be first affected by drought: their points will in consequence become rigid and inexpansible, and they will thence generally cease to elongate at an early period of the summer. The descending current of sap will be then employed in promoting the growth and elongation of those roots only, which are more favourably situate, and these comparatively with other parts of the tree, will grow rapidly [†]. Gravitation will direct these roots perpendicularly downward, and the tree will appear to have adopted the wisest and best plan of connecting itself with the ground: and it will really have employed the readiest means of doing so, as effectively as it could have done, if it had possessed all the feelings and instinctive passions and powers of animal life. The subsequent vigorous growth of such a tree is the natural consequence of an improved and more extensive pasture.

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[*] Phil. Trans. 1809, p. 169: or Journal, vol. xxv, p. 119.

[†] We do not find here, however, "the proper food and moisture," to "give ability to the root to attract and employ the descending or cortical current of sap." C.

Growth of the carrot and parsnep seeds explained in the first case,

When the seeds of the carrot and parsnep, in the experiments I have stated, were placed in a poor superficial soil, but which permitted the roots of the plants to pass readily through it, these were conducted downward by gravitation; while the plants grew feebly, because they received but little nutriment. The roots were in a situation analogous to that of the stems of trees in a crowded forest; and when the leading fibres of the roots came into contact with the rich mould, they acquired a situation correspondent to that of the leading branches of such trees, which are alone exposed to the light. The form of the roots of the plants was consequently long, slender, and cylindrical, like the stems of such trees. The roots of the one required the actual contact of proper soil and nutriment; and the branches of the other required the actual contact of light, to promote their growth.

and in the second.

When, on the contrary, the seeds of the preceding species of plants were placed in a rich superficial soil, their situation was analogous to that of a tree fully exposed, on every side, to the light; the branches of which would be extended, in every direction, immediately above the surface of the ground: and as the fibrous roots of the plants came into contact with the subsoil, which was not well calculated to promote their growth, their situation became analogous to that of shaded branches; and they consequently ceased to extend downwards. The fibrous roots of a tree, under similar circumstances, would have extended along the lower surface of the favourable soil; but after these roots had much increased in bulk, they would be found partly compressed into the subsoil, however poor and unfavourable, provided it contained no ingredients actually noxious.

Growth of trees requiring much or little moisture.

In obedience to similar laws, the roots of an aquatic tree will not extend freely in dry soil, nor those of a tree which requires but little moisture in a wet soil; and on this account the roots of the one will appear to have sought, and those of the other to have avoided, the contiguous water; though both, in the first period of their growth, pointed their roots alike in every direction.

When

Explanation of the growth of the beans, p. 372.

When the seeds of the bean, in the experiments I have described, were placed to vegetate beneath the mould of an inverted pot, a sufficient quantity of moisture was afforded by the mould, to occasion the protrusion of the radicles: but as soon as the under points of these had penetrated through the seed-coats, their surfaces were necessarily exposed to dry air, and were consequently rendered rigid and inexpansible; while their upper surfaces, being in contact with the moist mould, remained soft and expansible. If both the upper and lower surfaces of the radicles, at their points, had been equally well supplied with moisture, gravitation would have attracted the sap to the lower sides, where new matter would have been added; and the radicles would have extended perpendicularly downward, as in former experiments; but the influence of gravitation was, to a great extent, counteracted by the effects of drought upon the lower sides of the radicles, nearly as it was counteracted by centrifugal force, when made to act horizontally *.

As soon as the radicles had acquired sufficient age and maturity, efforts were made by them to emit fibrous roots; when want of proper moisture on the lower sides prevented their being protruded, in any other direction, except upwards. In this direction therefore they were alone emitted, (as I was confident that they would before I began the experiment) and having found proper food and moisture in the pots, they extended themselves upward through more than half the mould, which these contained.

The experiment repeated with additional moisture.

This experiment was repeated; and water was so constantly and abundantly given, that every part of the radicles was kept equally wet; and they then became perfectly obedient to gravitation, without being at all influenced by the mould above them.

Sulphates of alum, iron, and copper, did not check the growth of roots but by actual contact.

In other experiments, pieces of alum and of the sulphates of iron and copper were placed at small distances perpendicularly beneath the radicles of germinating seeds, of different species, to afford an opportunity of observing, whether any efforts would be made by them to avoid poisons; but they did not appear to be at all influenced,

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* Phil. Trans. 1806, p. 6: or Journal, vol. xiv, p. 411.

except

Food and moisture also require contact to promote growth.

except by actual contact of the injurious substances. The growth of their fibrous lateral roots was, however, obviously accelerated, when their points approached any considerable quantity of decomposing vegetable or animal matter: and when the growth of the roots was retarded by want of moisture, the contiguity of water, in the adjoining mould, though not apparently in actual contact with them, operated beneficially: but I had reason to suspect, that the growth of roots was, under these circumstances, promoted by actual contact with the detached and fugitive particles of the decomposing body, and of the evaporating water.

Growth and form of roots modified by the motion impressed on the tree by winds.

The growth and forms assumed by the roots of trees, of every species, are, to a great extent, dependent upon the quantity of motion, which their stems and branches receive from winds; for the effects of motion upon the growth of the root, and of the trunk and branches, which I have described in a former memoir, are perfectly similar*. Whatever part of a root is moved and bent by winds, or other causes, an increased deposition of alburnous matter upon that part soon takes place; and consequently the roots, which immediately adjoin the trunk of an insulated tree, in an exposed situation, become strong and rigid; while they diminish rapidly in bulk, as they recede from the trunk, and descend into the ground. By this sudden diminution of the bulk of the roots, the passage of the descending sap, through their bark, is obstructed; and it in consequence generates, and passes into many lateral roots; and these, if the tree be still much agitated by winds, assume a similar form, and consequently divide into many others. A kind of net-work, composed of thick and strong roots, is thus formed, and the tree is secured from the dangers, to which its situation would otherwise expose it.

Hence trees most exposed are rendered more secure;

while sheltered trees have only long and slender roots.

In a sheltered valley, on the contrary, where a tree is surrounded and protected by others, and is rarely agitated by winds, the roots grow long and slender, like the stem and branches, and comparatively much less of the circulating fluid is expended in the deposition of alburnum beneath the ground, and hence it not unfrequently happens; that a tree, in the most sheltered part of a valley, is uprooted; while the exposed and insulated tree, upon the adjoining mountain, remains uninjured by the fury of the storm.

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* Phil. Trans. 1803, p. 7.

Plants void of sensation and passion analogous to those of animal life.

In all the preceding arrangement, the wisdom of nature, and the admirable simplicity of the means it employs, are conspicuously displayed; but I am wholly unable to trace the existence of any thing like sensation or intellect in the plants: and I therefore venture to conclude, that their roots are influenced by the immediate operation and contact of surrounding bodies, and not by any degrees of sensation and passion analogous to those of animal life; and I reject the latter hypothesis, not only because it is founded upon assumptions, which cannot be granted, but because it is insufficient to explain the preceding phenomena, unless seedling plants be admitted to possess more extensive intellectual powers, than are given to the offspring of the most acute animal. A young wild-duck or partridge, when it first sees the insect upon which nature intends it to feed, instinctively pursues and catches it; but nature has given to the young bird an appropriate organization. The plant, on the contrary, if it could feel and perceive the objects of its wants, and will the possession of them, has still to contrive and form the organ by which these are to be approached. The writers, who have contended for the existence of sensation in plants, appear to have been sensible of the preceding and other obstacles, and have all betrayed the weakness of their hypothesis, in adducing a few facts only which are favourable to it, and waving wholly the investigation of all others.

In the description of the preceding experiments, I fear that I have been tediously minute; but, as I have selected a few facts only from a great number, which I could have adduced, I was anxious to give as accurate and distinct a view of those I stated, as possible.

I am, dear Sir,
with great respect,
sincerely yours,
THOS. AND. KNIGHT.

Downton, Jan. 15, 1811.

IX. *On*

IX.

On the mucilaginous State of Distilled Waters: by Mr. BUCHOLZ*.

Distilled waters apt to spoil.

IT is well known, that distilled waters spoil more or less quickly. They become mucilaginous, deposit a flocculent sediment, lose their smell and taste, and often acquire a fetid smell and putrid taste; all which appears to take place most frequently in waters destitute of essential oil.

Circumstances favourable to this.

It is known too, that this change proceeds more rapidly in proportion as the water contains but little oil; and if the distillation have been performed hastily, the flocculent sediment forms presently after, as in elder-flower water, linden water, &c.

They spoil in close and in open vessels.

Distilled waters spoil equally in open vessels, and in vessels closely stopped: but the change takes place more speedily in very close vessels.

We have two questions then to solve: What is the cause of this alteration? And what are the means of obviating it?

The oil supposed to be decomposed.

As waters distilled with the greatest care undergo this change, it may be suspected, that the oil is decomposed, and converted into mucilage †. Bauhoff's experiments tend to support this opinion. He dissolved in common distilled water, essential oil of peppermint, of fennel, of lemons, and of valerian. These waters, which were perfectly limpid, were kept in closely stopped bottles at the common temperature; and in a few weeks they became turbid, let fall a flocculent, mucilaginous sediment, and lost their smell.

But they may be fetid without the oil being destroyed.

A fetid smell however does not always indicate the total disappearance of the essential oil. Bauhoff examined some spoiled rosewater, that had been kept in a close vessel. The surface of this water was covered with a black pellicle, and its smell resembled that of sulphuretted hidrogen gas. Being left a few weeks exposed to the open air, the fetid smell vanished, and was replaced by that of roses*. Some rosewater, the putrefaction of which was very evident, recovered its smell by the addition of a little lime and iron.

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* Ann. de Chim. vol. lxiii, p. 90. Abridged from Tromsdorff's Pharmaceutical Journal, by Mr. Vogel.

† This appears inconsistent with what is said in the first paragraph. In the experiments of Bauhoff, that follow, as the term *dissolved* is employed, no doubt sugar or mucilage was used as an intermedium for uniting the oil and water. C.

It appears certain therefore, that the oils in distilled waters change their nature.

Waters, that have been distilled with too strong a heat, contain lest oil; which would seem to prove, that a part of it has undergone some sort of alteration.

Waters that contain no essential oil spoil.

But there are waters, that contain no essential oil, as those of elder-flowers, borage, nettles, &c. These waters probably carry up in distillation volatile odorant principles, which approach the nature of essential oils, and are decomposed still more easily.

The author's theory

But how are these principles converted into mucilage? As the flocculent matter forms more commonly in well stopped bottles, than when exposed to the air, this question may be easily answered. It is well known, that essential oils exposed to the air are converted into resins. This cannot be employed to explain the phenomenon. We must suppose then, that the oil, in passing to the state of mucilage, loses a part of its hidrogen; or that the oil becomes mucilage by uniting with one of the constituent principles of water, which however appears less probable. Perhaps it may be supposed, that the nitrogen of the air combines with the oil, or with the volatile odorant principles, to form mucilage.

Green matter in distilled water.

An analysis of the flocculent matter would tend to elucidate this point. However the remark made by Priestley and Sennebier would still remain to be explained. They both observed a green matter in distilled water exposed to the

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Freezing advantageous to distilled waters.

* Mr. Nachet, professor at the School of Pharmacy, long ago remarked, that distilled waters, which had been frozen, acquired a more powerful smell, and kept longer. He observed this to be particularly the case with balm, mint, and orange-flower water. *Vogel.*

The experiment of Bauhoff, given in the text, tends to confirm the opinion, that the change is *not* owing to a decomposition of the essential oil. C.

rays of the sun in vessels slightly covered. Sennebier found in this substance a number of small worms.

Best method of keeping distilled waters.

The second question remains, that of preventing distilled waters from spoiling. To prevent this inconvenience as far as possible, they should be kept in an airy cellar, in wide-mouthed vessels, covered with a paper. Once a month the paper should be taken off to renew the air at the surface.

It is advisable, to have these waters in the most concentrated state possible, so that their surface may be covered with a stratum of the volatile oil of the vegetable, which may afterward be separated by filtration. If the spoiling of distilled waters cannot wholly be prevented by these means, it may at least be deferred.

X.

A new Analysis of Ambergris: by Mr. BUCHOLZ *.

Various analyses of ambergris.

THE author first reviews the various analyses, that have been made of ambergris, and gives a comparative table of results obtained by modern chemists, namely by Rose, Juck, Bouillon-Lagrange, and Proust. He then subjected ambergris to the following experiments.

Its habitudes with water,

Water distilled from ambergris acquires a slight smell of this substance, without containing an oil.

alcohol,

Pure alcohol dissolves ambergris entirely, except a small quantity of black pulverulent matter. It dissolves a much larger quantity, if assisted by heat; and lets none fall on cooling. The liquor is then of a reddish brown †.

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* Ann. de Chim, vol. lxiii, p. 95. Abridged from Trommsdorff's Pharmaceutical Journal, by Mr. Vogel.

† When ambergris is treated with a small quantity of boiling alcohol, and the liquor filtered while hot, a yellowish white grumous substance is precipitated. If Mr. Bucholz did not observe this, it was probably owing to the small quantity, on which he operated. He employed only 20 grs. of ambergris to six drachms of alcohol, which he calls a saturated tincture. *Bouillon-Legrange.*

ether,

Ether dissolves it cold, and also leaves the black matter. This solution is not precipitated either by alcohol, or by water.

potash,

Caustic potash, whether dry or dissolved in water, combines very difficultly with one part of ambergris. This sparing solubility in potash may be employed as a test, to distinguish true ambergris from spurious.

and oils.

Oil of turpentine and oil of almonds dissolve ambergris very well, if assisted by heat.

Considered as a peculiar principle.

Instead of finding ambergris to be a compound of adipocere, resin, benzoic acid, and carbonaceous matter, agreeably to the results of Mr. Bouillon-Lagrange*, the author considers it as a substance *sui generis*. In the recapitulation of his paper, he expresses himself thus: "ambergris is a peculiar compound, which is a medium between wax and resin; differing from both in the manner in which it comports itself with alkalis; and approaching the resins, in alcohol dissolving a larger quantity of it than of wax, and in having a resinous aspect when it is cooled after having been melted. The author proposes to call it the *ambry principle* †."

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[*] See Journal, vol. vi, p. 179.

[†] If a drachm of ambergris be dissolved in two drachms of boiling alcohol, and the liquor filtered while hot, it lets fall on cooling that substance, which I have compared to adipocere, because it approaches it nearly in its properties. The supernatant fluid is rendered turbid by water, and reddens a weak infusion of litmus. This property is owing no doubt, as I have said in a former paper, to a small quantity of resin which it contains. I did not think it right therefore, to increase the number of new substances unnecessarily, by giving to the matter precipitated from the alcohol the name of *ambry principle*; I was satisfied with considering it as intermediate between resin and wax. As to the benzoic acid, I must confess, that I have found none in several specimens of ambergris, which I have analysed since. This leads to the suspicion, that there are manufactories of ambergris, as there are of castor.

Benzoic acid probably from sophistication.

XI. *Process*

XI.

Process for preparing pure Phosphoric Acid: by Mr. MARTRES, *Apothecary at Montauban, and Member of several Societies* *.

WHILE physicians devote their studies to the search after new means of alleviating the sufferings of mankind, it is the duty of apothecaries to second their efforts, by endeavouring to simplify or improve the preparation of medicines.

Improvements in pharmacy desirable.

Dr. Lasalle, of Montauban, having employed the phosphoric acid with success in the treatment of some diseases, I endeavoured to supply him with it very pure, in a little time, and without danger, by a process, which I have found to succeed completely: being fully convinced, that we cannot rely with entire confidence on preparations, that come to us in the ordinary way of trade.

Phosphoric acid used medicinally.

We may proceed in six different ways, to obtain phosphoric acid; but five of them are not very easy of execution, and the product is almost always contaminated with phosphorous acid.

Methods of preparing it.

The sixth, pointed out by Lavoisier, yields a pure phosphoric acid, but exposes the operator to some danger. This therefore it is desirable to obviate; which I have effected by means of an apparatus, that I shall describe before I give an account of my process.

Improved method.

The neck of a retort, placed on a sand-heat, is to be introduced into the mouth of a receiver, the second neck of this receiver into the mouth of another, and the second neck of this into a curved adopter, the mouth of which opens in a vessel of water, so as to answer the purpose of a tube of safety †.

Apparatus.

The apparatus being thus arranged, 32 gr. [494 grs.] of phosphorus are to be put into the retort, through its tubulure; and an equal weight of a mixture consisting of

Process described.

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* Ann. de Chim. vol. lxxiii, p. 99.

† The atmospheric air expelled from the receivers by the nitrous vapours escapes from the mouth of the adopter, which may afterward be stopped.

equal parts of concentrated nitric acid and distilled water. A tube of safety is then to be introduced into the same aperture, so that its toothed extremity shall reach the bottom of the retort. The apparatus is then to be luted, and dried.

The process is to commence with heating the sandbath till the liquor boils, and the phosphorus melts. A quantity of nitric acid is then to be poured into the funnel, sufficient to produce a level, without flowing into the retort. 8 gr. [123·5 grs.] of the same acid are then to be added; which, by their weight, force into the retort an equal quantity of the liquid, part of which still remains in the tube, without touching the phosphorus.

The phosphorus, retained at the bottom by its specific gravity, attracts the nitric acid, but, as it receives only a small quantity at a time, the combustion is slow, and effected without danger.

In proportion as the nitrous vapours in the retort diminish, a fresh portion of acid is to be poured into the funnel; and this is to be repeated, till the phosphorus is completely oxigenated.

Quantity of nitric acid required.

To effect the perfect combustion of 32 gr. [494 grs.] of phosphorus, I have employed 128 gr. [1977 grs.] of concentrated acid; or 192 gr. [2965·5 grs.] of what is commonly called fuming nitrous acid.

Results.

By operating in this manner, we obtain phosphoric acid, mixed with nitrous gas, and a quantity of superfluous liquid, from which it is to be freed by evaporation. This process takes more time with a retort, than it would with a matrass; but the operator is not exposed to inhale the nitrous gas. The liquid residuum should have the consistence of a thin sirup, and leave streaks on the glass, as milk or oil would do.

If the process I have just described produce pure phosphoric acid; if my simple and ready apparatus secure the operator from the nitrous fumes, and the accidents that might be occasioned by the explosion of the vessels; this apparatus and this process will no doubt be adopted, and perhaps not be thought uninteresting to those, who are engaged in chemistry.

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END OF THE THIRTIETH VOLUME.

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